UNIVERSITY OF ARIZONA
39001004859966

Digitized by the Internet Archive
in 2025

https://archive.org/details/systemofmechanic0004unse

A

SYSTEM

OF

MECHANICAL PHILOSOPHY.

By JOHN ROBISON, LL. D.

LATE PROFESSOR OF NATURAL PHILOSOPHY IN THE UNIVERSITY OF EDINBURGH.

WITH NOTES,

By DAVID BREWSTER, LL. D.

FELLOW OF THE ROYAL SOCIETY OF LONDON, AND SECRETARY TO THE ROYAL SOCIETY OF EDINBURGH.

IN FOUR VOLUMES,

AND A VOLUME OF PLATES.

VOL. IV.

EDINBURGH:

PRINTED FOR JOHN MURRAY, LONDON.

1822.

CONTENTS

OF

VOLUME FOURTH.

Page

CTRICITY 1

gnetism 205

ation of the Compass 354

perament of the Scale of Music 376

king Trumpet 452

ine Trumpet 486

ical Trumpet 501

ch-Work 539

nanship 610

ELECTRICITY.

We cannot but be somewhat surprised that, among the many attempts which have been made by the philosophers of Britain to explain the wonderful phenomena which are classed under the name of Electricity, no author of eminence, besides the Honourable Mr. Cavendish and Lord Mahon, have availed themselves of their susceptibility of mathematical discussion; and our wonder is the greater, because it was by a mathematical view of the subject, in the phenomena of attraction and repulsion, that the celebrated philosopher Franklin was led to the only knowledge of electricity that deserves the name of science; for we had scarcely any leading facts, by which we could class the phenomena, till he published his theory of *positive* and *negative*, or *plus* and *minus*, electricity. This is founded entirely on the phenomena of attraction and repulsion. These furnish us with all the indications of the presence of the mighty agent, and the marks of its kind, and the measures of its force. Mechanical force accompanies every other appearance; and this accompaniment is regulated in a determinate manner. Many of the effects of electricity are strictly mechanical,

producing local motion in the same manner as magnetism or gravitation produce it. One should have expected that the countrymen of Newton, prompted by his success and his fame, would take to this mode of examination, and would have endeavoured to deduce, from the laws observed in the action of this motive force, an explanation of other wonderful phenomena, which are inseparably connected with those of attraction and repulsion.

But this has not been the case, if we except the labours of the two philosophers above mentioned, and a few very obvious positions which must occur to all the inventors and improvers of electrometers, batteries, and other things of measurable nature.

This view has, however, been taken of the subject by a philosopher of unquestioned merit, Mr. Æpinus of the Imperial Academy of St. Petersburgh. This gentleman, struck with the resemblance of the electrical properties of the tourmalin to the properties of a magnet, which have always been considered as the subject of mathematical discussion, fortunately remarked a wonderful similarity in the whole series of electrical and magnetical attractions and repulsions, and set himself seriously to the classification of them. Having done this with great success, and having maturely reflected on Dr. Franklin's happy thought of plus and minus electricity, and his consequent theory of the Leyden phial, he at last hit on a mode of conceiving the whole subject of magnetism and electricity, that bids fair for leading us to a full explanation of all the phenomena; in as far, at least, as it enables us to class them with precision, and to predict what will be the result of any proposed treatment. He candidly gives it the modest name of a hypothesis.

This was published at St. Petersburgh in 1759, under the title of *Theoria Electritatis et Magnetismi*, and is unquestionably one of the most ingenious and brilliant performances of this century. It is indeed most surprising that it is so little known in this country. This, we imagine, has

been chiefly owing to the very slight and almost unintelligible account which Dr. Priestley has given of it in his history of electricity; a work which professes to comprehend every thing that has been done by the philosophers of Europe and America for the advancement of this part of natural science, and which indeed contains a great deal of instructive information, and, at the same time, so many loose conjectures and insignificant observations, that the reader reasonably believes that he has let nothing slip that was worthy of notice. We do not pretend to account for the manner in which Dr. Priestley has mentioned this work, so much, and so deservedly celebrated on the Continent. We cannot think that he has read it so as to comprehend it, and imagine, that seeing so much algebraic notation in every page, and being at that time a novice in mathematical learning, he contented himself with a few scattered paragraphs which were free of those embarrassments; and thus could only get a very imperfect notion of the system. The Hon. Mr. Cavendish has done it more justice in the 61st volume of the Philosophical Transactions, and considers his own most excellent dissertation only as an extension and more accurate application of Æpinus's Theory. That we have not an account of this exposition of the Franklinian theory of electricity in our language, is a material want in British literature; and we trust, therefore, that our readers will be highly pleased with having the ingenious discoveries of the great American philosopher put into a form so nearly approaching to a system of demonstrative science.

We propose, therefore, in this place, to give such a brief account of Æpinus's theory of electricity, as will enable the reader to reduce to a very simple and easily remembered law all the phenomena of electricity which have any close dependence on the mechanical effects of this powerful agent of nature; referring for a demonstration of what is purely mathematical to Sir Isaac Newton's Principia, and the Dissertation by Mr. Cavendish already mentioned, except in

such important articles as we think ourselves able to present in a new, and, we hope, a more familiar form. We do not mean, in this place, to give a system of philosophical electricity, nor even to narrate and explain the more remarkable phenomena. We confine ourselves to the phenomena which may be called *mechanical*, producing measurable motion as their *immediate* effect; and thus giving us a principle for the mathematical examination of the cause of electrical phenomena. We shall consider the reader as acquainted with the other physical effects of electricity, and shall frequently refer to them for proofs.

Moreover, as our intention is merely to give a synoptical view of this elaborate and copious performance of Mr. Æpinus, hoping that it will excite our countrymen to a careful perusal of so valuable a work, we shall omit most of the algebraic investigations contained in it, and present the conclusions in a more familiar, and not less convincing, form. At the same time we will insert the valuable additions made by Mr. Cavendish, and many important particulars not noticed by either of those gentlemen.

HYPOTHESIS OF ÆPINUS.

1. The phenomena of electricity are produced by a fluid of peculiar nature, and therefore called the ELECTRIC FLUID, having the following properties:

2. *First*, Its particles repel each other, with a force decreasing as the distances increase.

3. *Second*, Its particles attract the particles of some ingredient in all other bodies, with a force decreasing, according to the same law, with an increase of distance; and this attraction is mutual.

4. *Third*, The electric fluid is dispersed in the pores of other bodies, and moves with various degrees of facility

through the pores of different kinds of matter. In those bodies which we call *non-electrics*, such as water or metals, it moves without any perceivable obstruction ; but in glass, rosins, and all bodies called *electrics*, it moves with very great difficulty, or is altogether immoveable.

5. *Fourth*, The phenomena of electricity are of two kinds ; 1. Such as arise from the actual motion of the fluid from a body containing more into one containing less of it. 2. Such as do not immediately arise from this transference, but are instances of its attraction and repulsion.

These things being supposed, certain consequences necessarily result from them, which ought to be analogous to the observed phenomena of electricity, if this hypothesis be complete, or some farther modification of the assumed properties is necessary, in order to make the analogy perfect.

6. Suppose the body A (Plate I. fig. 1.) to contain a certain quantity of fluid. Its particles adjoining to the surface, such as P, are attracted by the particles of common matter in the body, but repelled by the other particles of the fluid. The totality of the attractive forces acting on P may be equal to the totality of the repulsive forces, or may be unequal. If these two sums are equal, P is in equilibrio, and has no tendency to change its place. But there may be such a quantity of fluid in the body, that the repulsions of the fluid exceed the attractions of the common matter. In this case, P has a tendency to quit the body, or there is an expulsive force acting on it, and it *will* quit the body if it be moveable. Because the same must be admitted in respect of every other particle of moveable fluid, it is plain that there will be an efflux, till the attraction of the common matter for the particles of fluid is equal to the repulsion of the remaining fluid. On the other hand, if the primitive repulsion of the fluid acting on the particle P be less than the attractions of the common matter, there will be the same, or at least a similar, superiority of attraction acting on the

fluid residing in the circumambient bodies; and there will be an influx from all hands, till an equilibrium be restored.

7. Hence it follows, that there may always be assigned to any body such a quantity of fluid that there shall be no tendency either to efflux or influx. But if the quantity be increased, and nothing prevent the motion, the redundant fluid will flow out; and if the proper quantity be diminished, there will be an influx of the surrounding fluid, if not prevented by some external force. This may be called the body's NATURAL QUANTITY; because the body, when left to itself, will always be reduced to this state.

8. If two bodies, A and B, contain each its natural quantity, they will not exert any sensible action on each other: for, because the fluid contained in B is united by attraction to the common matter, and is also repelled by the fluid in A, it necessarily follows, that the whole body B is repelled by the fluid in A. But, on the other hand, the matter in A attracts the fluid in B, and consequently attracts the whole body B: A similar action is exerted by B on A. These contrary forces are either equal, and destroy each other, or unequal, and one of them prevails. This equality or inequality evidently depends on the quantity of fluid contained in one or both of the bodies (§ 7.) Now it is known that bodies left entirely to themselves neither attract nor repel; and it follows from the hypothetical properties of the fluid, that if there be either a redundancy or deficiency of fluid, there will be an efflux or influx, till the attractions and repulsions balance each other. Therefore the internal state of two bodies which neither attract nor repel each other, is that where each contains its natural quantity of electric fluid.

9. In order, therefore, to conceive distinctly the state of a body containing its natural quantity, and to have a distinct notion of this natural quantity, we must suppose that the quantity of fluid competent to a particle of matter in A re-

pels the fluid competent to a particle of matter in B, just as much as it attracts that particle of matter; and also, that the fluid belonging to a particle of matter in A, repels the fluid belonging to a particle of matter in B, just as much as the particle of matter in A attracts it. Thus the whole fluid in the one repels the whole fluid in the other as much as it attracts the whole matter.

Since this must be conceived of every particle of common matter in a body, we must admit, that when a body is in its natural state, the quantity of electric fluid in it is proportional to the quantity of matter, every particle being united with an equal quantity of fluid. This, however, does not necessarily require that different kinds of matter, in their natural or saturated state, shall contain the same proportion of fluid. It is sufficient that each contains such a quantity, uniformly distributed among its particles, that its repulsion for the fluid in another body is equal to its attraction for the common matter in it. It is, however, more probable, for reasons to be given afterwards, that the quantity of electric fluid attached, or competent, to a particle of all kinds of matter is the same.

We shall now consider more particularly the immediate results of this hypothesis, in the most simple cases, from which we may derive some elementary propositions.

10. Since our hypothesis is accommodated to the fact, that bodies in their natural state, having their natural quantity of electric fluid, are altogether inactive on each other, by making this natural quantity such, that its mutual repulsion exactly balances its attraction for the common matter—it follows, that we must deduce all the electric phenomena from a redundancy or deficiency of electric fluid. This accordingly is the Franklinian doctrine. The redundant state of a body is called by Dr. Franklin POSITIVE OR PLUS ELECTRICITY, and the deficient state is called NEGATIVE OR MINUS ELECTRICITY.

A body may contain more than its natural quantity, or less, in every part, or it may be redundant in one place and deficient in another. These different conditions will exhibit different appearances, which must be considered first of all.

11. Let the body A (fig. 1.) be supposed in its natural state throughout, which we shall generally express by saying that it is SATURATED; and let us express the quantity of fluid required for its saturation by the symbol Q. Let P be a superficial particle of the fluid. It is attracted by the common matter of the body, (which we shall in future call simply the *matter*,) and it is repelled equally by the fluid. Let us call the attraction a, and the repulsion r. Then the force with which the superficial particle is attracted by the body, must be $= a - r$, and $a - r$ must be $= 0$, because $a = r$. Let the quantity f of fluid be added to the body, and uniformly distributed through its substance. Then, because we must admit that the action is in proportion to the quantity of acting fluid, and this is now $Q + f$, we have $Q : Q + f = r : \frac{\overline{Q+f} \times r}{Q}$; and therefore P is repelled by the whole fluid with the force $\frac{\overline{Q+f} \times r}{Q}$, or $\frac{Q\,r}{Q} + \frac{f\,r}{Q}$, or $r + \frac{f\,r}{Q}$. But it is attracted by the common matter in the same manner as before, that is, with a force $= a$. Therefore the whole action on P is $= a - r - \frac{f\,r}{Q}$. But $a - r = 0$. Therefore the whole action on P is $-\frac{f\,r}{Q}$; that is, P is repelled with the force $\frac{f\,r}{Q}$.

This will perhaps be as distinctly conceived by recollecting, that as much of the fluid as was necessary for saturation, that is, the quantity Q, puts the particle P in equilibrio; and therefore we need only consider the action of the redundant fluid f. To find the repulsive force of this, say

$Q : f = r : \frac{fr}{Q}$, and prefix the sign —; because we are to consider attractions as positive, and repulsions as negative, quantities.

12. Unless, therefore, the particle P be withheld by some other force, it will quit the body, being expelled by a force $\frac{fr}{Q}$. And as every superficial particle is in a similar situation, we see that there will be an efflux from an overcharged body, till all the redundant fluid has quitted it. This efflux will indeed gradually diminish as the expelling force $\frac{fr}{Q}$ diminishes; that is, as f diminishes, but will never cease till f be reduced to nothing. But if there be either an external force acting on the superficial fluid in the opposite direction, or some internal obstruction to its motion, the efflux will stop when the remaining expelling force is just in equilibrio with this external force, or this obstruction.

13. On the other hand, if the body contains less than its natural quantity of fluid, there will be an influx from without; for if there be a deficiency of fluid $= f$, the particle P will be repelled with the force $\frac{\overline{Q-f} \times r}{Q}$, $= r - \frac{fr}{Q}$. It is attracted with the force a; and therefore the whole action is $= a - r + \frac{fr}{Q}$, $= + \frac{fr}{Q}$ (because $a - r = 0$); that is, P is attracted with the force $\frac{fr}{Q}$. Fluid will therefore enter from all quarters, as long as there is any deficiency of the quantity necessary for saturation, unless it be opposed by some external force, or hindered by some internal obstruction.

When there is a deficiency of fluid, there is a redundancy of matter, such that its attraction for external fluid is equal to the repulsion of a quantity f of fluid. This confirms the

assumption in § 10, that *the action of a body on the electric fluid depends entirely on the redundant fluid, or the redundant matter of the body.*

14. The efflux or influx may be prevented, either by surrounding the body with substances, through the pores of which the fluid cannot move at all, or by the body itself being of this constitution. And thus we see, that the very circumstance of being impervious to the fluid, or completely permeable, renders the body capable or incapable of permanently exhibiting electrical phenomena, if surrounded by permeable bodies. This circumstance alone, therefore, is sufficient to constitute the difference between *electrics per se*, and *non electrics*. —Here, then, is a numerous class of phenomena, which receive an explanation by this hypothetical constitution of the electric fluid. All electrics *per se* are bodies fit for confining electricity in bodies which are rendered capable (by whatever means) of producing electrical phenomena ; and no conductor, or substance which allows the electricity to pass through it, can be made electric by any of the means which produce that effect in *insulators*. And it is well known, that the electricity of electrics is vastly more durable than that of non electrics in similar situations. It is true, indeed, that an electric, which has been excited so as to exhibit electric phenomena with great vivacity, loses this power very quickly if plunged into water, or any other conducting body. But this is owing to the redundancy or deficiency being quite superficial, so that the parts which are disposed to give out or to take in the fluid are in immediate contact with the conducting matter. That the redundancy or deficiency is superficial, follows from this hypothesis ; for when the surface is overcharged by the means employed for exciting, the impermeability of the electric *per se* prevents this redundant fluid from penetrating to any depth ; and when the surface has been rendered deficient in fluid, the same impermeability prevents the fluid from expanding from the interior parts, so as to contribute to the replenishing the superficial stratum with fluid.

If, indeed, we could fall on any way of overcharging the interior parts of a glass ball, or of abstracting the natural quantity from them, it is highly probable, that it would continue to attract or repel even after it had been plunged in water. Although the surrounding water would instantly take off the fluid redundant contained in the very surface, the repulsion of the fluid in the internal parts would still be sensible: nay, if a very small permeability be supposed, the body would again become overcharged at the surface; just as we see, that when we plunge a red hot ball of iron into water, and take it out again immediately, it is black on the surface, and may be touched with the finger; but in half a minute after, it again becomes red hot. Perhaps this may be accomplished with a globe of sealing wax, which is permeable while liquid, by electrifying it in a particular way while in that state, and allowing it to freeze. But the reader is not far enough advanced in the hypothesis to understand the process which must be followed. He cannot but recollect, however, many examples in coated glass, &c. where the electricity is most pertinaciously retained by a surface in very close contact with conductors.

15. Let us now suppose a body N S (fig. 2.), containing in the half NA a quantity f of redundant fluid, and in the half AS let there be a deficiency g of fluid; that is, let there be a quantity of matter unsaturated, and such as will attract fluid as much as the quantity g of fluid would repel it. Let the fluid necessary for the saturation of each half of NS be Q, as before. Let the attraction of the whole matter of NA for a particle of fluid at N be a; and let r be the repulsion exerted on the same particle N by the whole uniformly distributed fluid in NA, and let r' be the repulsion exerted by the *same quantity* of fluid in the remote part SA. Then the force with which the particle N or S is attracted by the merely saturated body NS must be $= a - r - r'$. This is evidently nothing, if the body be in its natural state. But as NA contains the redundant fluid f, and SA is deficient by

the quantity g, the whole action must be $a - \frac{\overline{Q+f} \times r}{Q}$ $- \frac{\overline{Q-g} \times r'}{Q}$. But because $a - r - r' = o$, the action becomes $= \frac{g r' - f r}{Q}$, or because r is greater than r', the particle N is repelled with the force $\frac{f r - g r'}{Q}$. In like manner, the particle S is attracted with the force $\frac{g r - f r'}{Q}$.

16. In the mean time, a particle C, situated at the middle, must be in equilibrio, if the body be in its natural state, being equally attracted, and also equally repelled, on both sides. But as we suppose that NA is overcharged with the quantity f, C must be repelled in the direction CS with the force $\frac{f r}{Q}$. And if we also suppose that AS is deficient by the quantity g, C is attracted in the direction CS with a force $\frac{g r}{Q}$. Therefore, on the whole it is urged in the direction CS with the force $\frac{f r + g r}{Q}$, or $\frac{\overline{f+g} \times r}{Q}$.

17. Hence we learn, that as long as there is any redundancy in AN, and deficiency in AS, there is a tendency of the redundant fluid to move from N towards S; and, if the body be altogether permeable by the electric fluid, we cannot have a permanent state till the fluid is similarly distributed, and equally divided, between the two halves of NS. Therefore a state like that assumed in this example cannot be permanent in a conducting body, unless an external force act on it; but it may subsist in a non-conductor, and in a lesser degree, in all imperfect conductors.

18. It is necessary, in this place, to consider a little the nature of that resistance which must be assigned to the motion of the electric fluid through the pores of the body. If it resemble the resistance opposed by a perfect fluid, arising

solely from the inertia of its particles, then there is no inequality of force so minute but that it will operate a uniform distribution of the fluid, or at least a distribution which will make the excess of the mutual attractions and repulsions precisely equal and opposite to the external force which keeps it in any state of unequal distribution. But it may resemble the resistance to the descent of a parcel of small shot disseminated among a quantity of grain, or the resistance to motion through the pores of a plastic or ductile body, such as clay or lead. Here, in order that a particle may change its place, it must overcome the tenacity of the adjoining particles of the body. Therefore, when an unequal distribution has been produced by an external force, the removal or alteration of that force will not be followed by an equable distribution of the fluid. In every part there will remain such an inequality of distribution, that the want of equilibrium between the electric attractions or repulsions is balanced by the tenacity of the parts.

19. We learn farther from the foregoing propositions, that a particle at N is less repelled than if the part AS were overcharged as AN is: for in that case, it would be expelled by a force $\frac{f \times \overline{r+r'}}{Q}$, which is much greater than $\frac{fr-gr}{Q}$. And, in like manner, the particle S is attracted with less force than it would be if NA were equally undercharged with SA.

20. The condition of the body now described may be changed by different methods. The redundant fluid in AN may flow into AS, where it is deficient, till the whole be uniformly distributed; or fluid may escape from AN, and fluid may enter into AS, till the body be in its natural state. The first method will be so much the slower as the body is less permeable, or more remarkably *electric per se;* and the second method will be slower than if the whole body were overcharged or undercharged.

21. What we have been now saying of a body NS that is overcharged at one end, and undercharged at the other, and

capable of retaining this state, is applicable, in every particular, to two conducting bodies NA and SA′, having a non-conducting body Z interposed between them, as in fig. 3. All the formulas, or expressions of the forces which tend to expel or to draw in fluid, are the same as before. Perhaps this is the best way of forming to ourselves a distinct notion of the body that is redundant in fluid at one end, and deficient at the other. And we perceive, that the state of the two bodies, separated by the electric Z, will be more permanent when one is overcharged, and the other undercharged, than if both are either over or undercharged.

22. It must be remarked, that the quantities f and g were taken at random. They may be so taken, that the force with which the fluid tends to escape at N, or to enter at S, may be nothing, or may even be changed to their opposite. Thus, in order that there may be no tendency to escape from N, we have only to suppose $gr' - fr = 0$, or $g : f = r : r'$, and $g = \frac{fr}{r'}$. In this case, the particle at N is as much attracted by the redundant matter in SA as it is repelled by the redundant fluid in NA.

23. When the extremity N is rendered inactive in this manner, the condition of the other extremity S is considerably changed. To discover this condition, put $\frac{fr}{r'}$ in place of g in the formula $\frac{gr - fr'}{Q}$, which expresses the attraction for a particle at S, and we obtain $\frac{f \times \overline{r^2 - r^2}}{Q r'}$.

24. On the other hand, we may have the redundancy and deficiency so balanced, that there shall be no tendency to influx at S. For this purpose, we must make $g = \frac{fr'}{r}$. When this obtains at S, the action at N will be had by putting $\frac{fr'}{r}$ in place of g in the formula $\frac{fr - gr'}{Q}$, and this will give $\frac{f \times r'^2 - r^2}{Q r}$ for the force repelling a particle at N.

25. When the tendency to efflux or influx is induced in this manner, by a due proportion of the redundancy and deficiency of electric fluid, the part of the body where this obtains is by no means in its natural state, and may contain either more or less than its natural quantity. But it neither acts like an overcharged nor like an undercharged body, and may therefore be called NEUTRAL. The reader who is conversant with electrical experiments, will recollect numberless instances of this, and will also recollect that they are important ones. Such, for example, is the case with the plates and covers of the electrophorus. These circumstances, therefore, claim particular attention.

26. As the quantities f and g may be so chosen, that the apparatus shall be *neutral*, either at S or at N; they may likewise be so, that either end shall exhibit either the appearance of *redundancy* or *deficiency*. Thus, instead of neutrality at N, we may have repulsion, as at the first, by making g less in any degree than $\frac{fr}{r'}$. If, on the contrary, g be greater than $\frac{fr}{r'}$, the extremity N, though overcharged, will attract fluid. In like manner, if g be less than $\frac{fr'}{r}$, the extremity S, although undercharged, will repel fluid.—We may make the following general remarks.

27. *First*, Both extremities N and S cannot be neutral at the same time: for since the neutrality arises from the increased quantity of redundancy or deficiency at the other extremity, so as to compensate for its greater distance, the activity of that extremity must be proportionably greater on the fluid adjoining to its surface, whether externally or internally. When an overcharged extremity is rendered neutral, the other extremity attracts fluid more strongly; and when a deficient extremity is rendered neutral, the other repels fluid more strongly. All these elementary corollaries will be fully verified afterwards, and give clear explanations of the most curious phenomena.

Second, We have been supposing, that the redundant fluid is uniformly spread, and that the body is divided into equal portions; but this was merely to simplify the procedure and the formulæ. The reader must see, that the general conclusions are not affected by this, and that similar formulæ will be obtained, whatever is the disposition of the fluid. We cannot tell in what manner the redundant fluid is disposed, even in a body of the simplest form, till we know what is the variation of its attraction and repulsion by a change of distance; and even when this has been discovered, we find it difficult in most cases, and impossible in many, to ascertain the mode of distribution. We shall learn it in some important cases, by means of various phenomena judiciously selected.

28. A body may be considered in many divisions, in some of which the fluid is redundant, and in others deficient. We may express the repulsion of the whole of this body in the same way as we express that of a body considered in two divisions, using the letters f, g, h, &c. to express the quantities of redundant or deficient fluid in each portion, while Q expresses the quantity necessary for saturating each of them; and the repulsion at different distances may be expressed by r, r', r'', r''', &c. as they are more and more remote; and we may express their action as attractive or repulsive by prefixing the sign + or —. Thus the attraction may be $\frac{(fr - gr' + hr'' - ir''')}{Q}$, &c.

29. Having obtained the expressions of the invisible actions of electrified bodies on the fluid within them, or surrounding them, let us now consider their sensible actions on other bodies, producing motion, or tendencies to motion.

Here it is obvious, that the mechanical phenomena exhibited are what may be called *remote* EFFECTS of the acting forces. The immediate effects, or the mutual actions of the particles, are not observed, but hypothetically inferred. The tangible matter of the body is put in motion, in consequence

of its connection with the fluid residing in the body, which fluid is the only subject of the action of the other body.

In considering these phenomena, we shall content ourselves with a more general view of the actions which take place between the fluid or tangible matter of the one body, and the fluid or matter of the other, so as to gain our purpose by more simple formulæ than those hitherto employed. They were premised, however, because we *must* have recourse to them on many very important particular occasions.

30. Let there be two bodies, A and B, in their natural state. Let the tangible matter in A be called M, and let the fluid necessary for its saturation be called F, and let m and f be the tangible matter and the fluid in B. Let the mutual action between a single particle of fluid and the matter necessary for its saturation be expressed by the indeterminate symbol z, because it varies by a change of distance.

The actions are mutual and equal. Therefore when the motion of B by the action of A is determined, the motion of A is also ascertained. We shall therefore only consider how A is affected. 1. Every particle of fluid in A tends toward every particle of matter in B with the force z. The whole tendency of A toward B may therefore be expressed by z, multiplied by the product of F and m. 2. Every particle of fluid in A is repelled by every particle of fluid in B, with the same force z. 3. Every particle of matter in A is attracted by every particle of fluid in B, with the same force. We may express this more clearly and briefly thus:

1. F tends *toward* m with the force $+ \mathrm{F}\, m\, z$
2. F tends *from* f with the force $- \mathrm{F}\, f\, z$
3. M tends *toward* f with the force $+ \mathrm{M}\, f\, z$

Therefore the sensible tendency of A to or from B will be $= z \times \overline{\mathrm{F}\, m + \mathrm{M}\, f - \mathrm{F}\, f}$. But, by the hypothesis, the attraction of a particle of the fluid in A for a particle of the matter in B, is equal to its repulsion for the particle or par-

cel of the fluid attached or competent to that particle of matter. Therefore the attraction F $m z$ is balanced by the repulsion F $f z$. Therefore there remains the attraction of the matter in A for the fluid in B unbalanced, and the body A will tend toward the body B, with the force M $f z$, or B attracts A with the force M $f z$. A must therefore move toward B. And, by the 3d law of motion, B must move toward A with equal force.

31. But the fact is, that no tendency of any kind is observed between bodies in their natural state. The hypothesis therefore, is not complete. If we abide by it, as far as it is already expressed, we must farther suppose, that there is some repulsive force exerted between the bodies to balance the attraction of M for f. Mr. Æpinus, therefore, supposes, that every particle of tangible matter repels another particle as much as it attracts the fluid necessary for its saturation. The whole action of B on A will now be $= z \times \overline{\text{F } m - \text{F } f - \text{M } m + \text{M } f}$. F $m z$ is balanced by F $f z$, and M $m z$ by M $f z$, and no excess remains on either side.

32. Æpinus acknowledges, that this circumstance appeared to himself to be hardly admissible; it seeming inconceivable, that a particle in A shall repel a particle in B, or tend from it, electrically, while it attracts it, or tends toward it, by planetary gravitation. We cannot conceive this; but more attentive consideration shewed him, that there is nothing in it contrary to the observed analogy of natural operations. We must acknowledge, that we see innumerable instances of inherent forces of attraction and repulsion; and nothing hinders us from referring this lately discovered power to the class of primitive and fundamental powers of nature. Nor is there any difficulty in reconciling this repulsion with universal gravitation; for while bodies are in their natural state, the electric attractions and repulsions precisely balance each other, and there is nothing to disturb the phenomena of planetary gravitation; and when bodies are not in their natural electrical state, it is a fact that their gravitation is disturbed. Although we cannot conceive a body to have a

tendency to another body, and at the same time a tendency from it, when we derive our notion of these tendencies entirely from our own consciousness of effort, endeavour, *conatus*, *nisus accedendi seu recedendi*, nothing is more certain than that bodies exhibit at once the appearances which we endeavour to express by these words. We can bring the north poles of two magnets near each other, in which case they recede from each other; and if this be prevented by some obstacle, they press on this obstacle, and seem to endeavour to separate. If, while they are in this state, we electrify one of them, we find that they will now approach each other; and we have a distinct proof that both tendencies are in actual exertion by varying their distances, so that one or other force may prevail; or by placing a third body, which shall be affected by the one but not by the other, &c. We do not understand, nor can conceive in the least, how either force, or how gravity, resides in a body; but the effects are past contradiction. It must be granted, therefore, that this additional circumstance of Æpinus's hypothesis has nothing in it that is repugnant to the observed phenomena of nature.

N.B. It is not necessary to suppose (although Mr. Æpinus does suppose it), that every atom of tangible matter repels every other atom. It will equally explain all the phenomena, if we suppose that every particle contains an atom or ingredient having this property, and that it is this atom alone which attracts the particles of electrical fluid. The material atoms having this property, and their corresponding atoms of fluid, may be very few in comparison with the number of atoms which compose the tangible matter. Their mutual specific action being very great in comparison with the attraction of gravitation (as we certainly observe in the action of light), all the phenomena of electricity will be produced without any sensible effect on the phenomena of gravitation, even although neither the electric fluid nor its ally, this ingredient of tangible matter, should not gravitate. But this supposition is by no means necessary.

Since we call that the natural electrical state of bodies in which they do not affect each other, and the hypothetical powers of the fluid are accommodated to this condition, we may consider any body that has more than its natural quantity as consisting of a quantity of matter saturated with fluid, and a quantity of redundant fluid superadded; and an undercharged body may be considered as consisting of a quantity of matter superadded. The saturated matter of these two bodies will be totally inactive on another body in its natural state, and will neither attract nor repel it, nor be attracted nor repelled by it; therefore the action of the overcharged body will depend entirely on the redundant fluid; and that of the undercharged body will depend entirely on the redundant matter; therefore we need only consider them as consisting of this redundant fluid or matter, agreeably to what was said in more vague terms in § 10. and 13. This will free us from the complicated formulæ which would otherwise be necessary for expressing all the actions of the fluid and tangible matter of two bodies on each other. The results will be sufficiently particular for distinguishing the sensible action of bodies in the chief general cases; but in some particular and important cases, it is absolutely necessary to employ every term.

33. *First*, Suppose two bodies A and B, containing the quantities F' and f' of redundant fluid, it is plain that their mutual action is expressed by $F' \times f' + z$, and that it is a repulsion; for since every particle of redundant fluid in A repels every particle of redundant fluid in B with the force z; and since F' and f' are the numbers of such particles in each, the whole repulsion must be expressed by the product of these numbers.

34. *Second*, In like manner, two bodies A and B, containing the redundant matter M' and m', will repel each other with the force $M' m' z$.

35. *Third*, And two bodies A and B, one of which A contains the redundant fluid F', and the other B contains the

redundant matter m, will attract each other with the force $F\ m'\ z$.

36. *Fourth*, It follows from these premises, that if either of the bodies be in its natural state, they will neither attract nor repel each other; for, in such a case, one of the factors F', or f', or M', or m', which is necessary for making a product, is wanting. This may be perceived independent of the mathematical formula; for if A contain redundant fluid, and B be in its natural state, every particle of the redundant fluid in A is as much repelled by the natural fluid in B as it is attracted by the tangible matter.

37. The three first propositions agree perfectly with the known phenomena of electricity; for bodies repel each other, whether both are positively or both are negatively electrified, and bodies always attract each other when the one is positively and the other negatively electrified. But the fourth case seems very inconsistent with the most familiar phenomena. Dr Franklin and all his followers assert, on the contrary, that electrified bodies, whether positive or negative, always attract, and are attracted, by all bodies which are in their natural state of electricity. But it will be clearly shewn presently, that they are mistaken, and that Franklin's theory necessarily supposes the truth of the fourth proposition, otherwise two bodies in their natural state could not be neutral or inactive, as any one may perceive on a very slight examination by the Franklinian principles. It will presently appear, with the fullest evidence; and, in the mean time, we proceed to explain the action of bodies which are overcharged in some part, and undercharged in another.

38. Let the body B (fig. 4.) be overcharged in the part B n, and undercharged in the part B s, and let f' and m be the redundant fluid and common matter in those parts; let A be overcharged, and contain the redundant fluid F'; let z and z' express the intensity of action corresponding with the distances of A from the overcharged and undercharged parts of B; the part B n repels A with the force $F'f'z$,

while the part BS attracts it with the force $F' m' z$: A will therefore be attracted or repelled by B, according as $F m' z'$ is greater or less than $F f' z'$; that is, according as $m' z$ is greater or less than $f' z$. This, again, depends on the proportion of f' to m', and on the proportion of z to z'. The first depends on many external circumstances, which may occasion a greater or less redundancy or deficiency of electrical fluid; the second depends entirely on the law of electric attraction and repulsion, or the change produced in its intensity by a change of distance. As we are, at present, only aiming at very general notions, it is enough to recollect, that all the electric phenomena, and indeed the general analogy of nature, concur in shewing that the intensity of both forces (attraction and repulsion) decreases by an increase of distance; and to combine this with that circumstance of the hypothesis which states the repulsion to be equal to the attraction at the same distance; therefore both forces vary by the same law, and we have z always greater than z'. The visible action of B on A (which, by the 3d law of motion, is accompanied by a similar action of A on B) may be various, even with one position of B, and will be changed by changing this position.

First, We may suppose that B contains, on the whole, its natural quantity, but that part of it is abstracted from BS, and is crowded into BN. This is a very common case, as we shall see presently, and it will be expressed in our formula, by making $f' = m'$. In this case, therefore, we have $F' f' z$ greater than $F' m' z$, because z is greater than z'. A will therefore be repelled by B, and will repel it; and the repulsion will be $F' f' \times \overline{z - z'}$.

It is evident, that if A be placed on the other side of B, the appearances will be reversed, and the bodies will attract each other with the force $F' f' \times \overline{z - z'}$.

It is also plain, that if A be as much undercharged as we have supposed it overcharged, all the appearances will be reversed; if on the undercharged side of B, it will be repelled; and if on the overcharged side of B, it will be attracted.

39. *Second*, If the redundancy and deficiency in the two portions of B be inversely proportional to the forces, so that $F' : m' = z' : z$, we shall have $f' z = m' z'$, and $m' = \frac{f' z}{z'}$. In this case these two actions balance each other, and A is neither attracted nor repelled when at this precise distance from the overcharged side of B. B may be said to be NEUTRAL with respect to A, although A and the adjoining side of B are both overcharged.

40. But if A be placed at the same distance on the other side of B, the effect will be very different: For because $m' = \frac{f' z}{z'}$, and $m' z'$ is now changed into $m' z$, and $f' z$ into $f z'$, we have the action on $A = F' \times \left(\frac{f' z}{z'} - f' z\right), = F' f' \times \frac{z_2 - z'^2}{z'}$; that is, A is strongly attracted.

In like manner, f' and m' may be so proportioned, that when A, containing redundant fluid, is placed near the undercharged end of SB, it shall neither be attracted nor repelled, B becoming neutral with regard to A at that precise distance. For this purpose m' must be $= \frac{f z'}{z}$. And if A be now placed at the same distance on the other side of B, it will be repelled with the force $F' f' \frac{\times z^2 - z'^2}{z}$.

Thus, when the overcharged end is rendered neutral to an overcharged body, the other end strongly attracts it; and when the undercharged end is rendered neutral to the same body, the overcharged end strongly repels it.

Similar appearances are exhibited when A is undercharged.

These cases are of frequent occurrence, and are important, as will appear afterwards.

41. It is easy now to see what changes will be made on the action of B on A, by changing the proportion of f' and m'. If m' be made greater than $\frac{f' z}{z'}$, A will be attracted in

the situation where it was formerly neutral; and if m' be made less, A will be repelled, &c. &c.

Therefore, when we observe B to be neutral, or attractive, or repulsive, we must conclude that m' is equal to $\frac{f z}{z'}$, or greater or less than it, &c.

We have been thus minute, that the reader may perceive the agreement between this action on a body containing redundant fluid, and the action on the superficial fluid formerly considered in § 21, 22, 23, 24. When these things are attended to, we shall explain, with great ease, all the curious phenomena of the electrophorus.

42. There is another circumstance to be attended to here, which will also explain some electrical appearances that seem very puzzling. We limited the inactivity of B to a certain precise distance of the body A. This inactivity required that m' should be $= \frac{f z}{z'}$. If A be brought nearer, both z and z' are increased. If they are both increased in the same proportion, the value of $\frac{z}{z'}$ will be the same as before, and the body A will neither be attracted nor repelled at this new distance. But if z increase faster than z', we shall have $f' z$ greater than $m z'$, and A will be repelled; and if z increases more slowly than z', A will be attracted by bringing it nearer. The contrary effects will be observed if A be removed farther from the overcharged end of B. This explains many curious phenomena: and those phenomena become instructive, because they enable us to discover the law of electric action, by shewing us the manner in which it diminishes by a change of distance. Electricians cannot but recollect many instances, in which the motion of the electrometer appeared very capricious. The general fact is, that when an overcharged pith ball is so situated near the overcharged side of the electrophorus as to be neutral, it is repelled when brought nearer, but attracted when removed to a great dis-

tance. This shews that z increases faster than z' when A is brought nearer to B. Now, since the bodies may be again rendered neutral at a greater distance than before, and the same appearances are still observed, it follows, that the law of action is such, that *every* diminution of distance causes z to increase faster than z'. We shall find this to be valuable information.

43. Let us, in the last place, inquire into the sensible effect on A when it also is partly overcharged and partly undercharged. This is a much more complicated case, and is susceptible of great variety of external appearances, according to the degrees of redundancy and deficiency, and according to the kind of electricity (positive or negative) of the ends which front each other.

44. First, then, let the overcharged end of A (fig. 5.) front the undercharged end of B, they being overcharged in N and n, but undercharged in S and s. Let F and f be the quantity of fluid natural to each; and let F′ and f' be the redundancy in N and n, and M′ and m' the deficiency in S and s. Moreover, let Z and Z′ represent the intensity of actions of a particle in N on a particle in n and s; and let z and z' represent the actions of a particle in S on a particle in n and in s; or, in other words, let Z, Z′, z, z', represent the intensity of action between particle and particle, corresponding to the distances N s, N n, S s, S n.

Proceeding in the same manner as in the former examples, we easily see, that the action of B on A is $= \frac{\mathrm{F}'\,m'\,\mathrm{Z} - \mathrm{F}'\,f'\,\mathrm{Z}' - \mathrm{M}'\,m'\,z + \mathrm{M}'\,f'\,z'}{\mathrm{F}\,f}$; the attractions are considered as positive quantities, having the sign + prefixed to them, and the repulsions are negative, having the sign —.

This action will be either attractive or repulsive, according as the sum of the first and last terms of the numerator exceeds or falls short of the sum of the second and third: And the value of each term will be greater or less, accord-

ing to the quantity of redundant fluid and matter, and also according to the intensity of the electric action. It would require several pages to state all those possible varieties. We shall therefore content ourselves at present with stating the simplest case; because a clear conception of this will enable the reader to form a pretty distinct notion of the other possible cases; and also, because this case is very frequent, and is the most useful for the explanation of phenomena.

We shall suppose, that the redundant part of each body is just as much overcharged as the deficient part is undercharged; so that $F' = M'$, and $f' = m'$. In this case, the formula becomes $\frac{F' f' (Z - Z' - z + z')}{F f}$.

Here we see that the sensible or external effect on A depends entirely on the law of electric action, or the variation of its intensity by a change of distance. If the sum of Z and z' exceed the sum of Z' and z, A will be attracted; but if $Z' + z'$ be less than $Z' + z$, A will be repelled. This circumstance suggests to us a very perspicuous method of expressing these actions between particle and particle, so that the imagination shall have a ready conception of the circumstance which determines the external complicated effect of this internal action. This will be obtained by measuring off from a fixed point of a straight line portions respectively equal to the distances N s, N n, S s, and S n, between the points of the two bodies A and B, where we suppose the forces of the redundant fluid and redundant matter to be concentrated, and erect ordinates having the proportion of those forces. If the law of action be known, even though very imperfectly, we shall see, with one glance, of which kind the movements or tendencies of the bodies will be. Thus, in fig. 5, drawing the line C z, take C p = N s, C q = N n, C r = S s, and C t = S n, and erect the ordinates P p, Q q, R r, and T t. If the electric action be like all the other attractions and repulsions which we are familiarly acquainted with, decreasing with an increase of distance, and

decreasing more slowly as the distances are greater, these ordinates will be bounded by a curve PQRTZ, which has its convexity turned toward the axis. We shall presently get full proof that this is the case here; but we premise this general view of the subject, that we may avoid the more tedious, but more philosophical, process of deducing the nature of the curve from the phenomena now under consideration.

45. This construction evidently makes the pair of ordinates P p, Q q, equidistant with the pair R r, T t. Also, P p, R r, and Q q, T t, are equidistant pairs. It is no less clear, that the sum of P p and T t, exceeds the sum of Q q and R r. For if C z be bisected in V, and V v be drawn perpendicular to it, cutting the straight lines PT and QR in x and y, then $x\,v$ is the half sum of P p and T t, and $y\,v$ is the half sum of Q q and R r. Moreover, if Q m and T n are drawn parallel to the base, we see that P m exceeds R r; and, in general, that if any pair of equidistant ordinates are brought nearer to C, their difference increases, and *vice versa*. Also, if two pairs of equidistant ordinates be brought nearer to C, each pair by the same quantity, the difference of the nearest pair will increase more than the difference of the more remote pair. And this will hold true, although the first of the remote pair should stand between the two ordinates of the first pair. If the reader will take the trouble of considering these simple consequences with a little attention, he will have a notion of all the effects that are to be expected in the mutual actions of the two bodies, sufficiently precise for our present purpose. We shall give a much more accurate account of these mathematical truths in treating the subject of MAGNETISM, where precision is absolutely necessary, and where it will be attended with the greatest success in the explanation of phenomena.

46. Now let us apply this to our present purpose. *First*, then, When the overcharged end of A is turned toward the

undercharged end of B, A must be attracted; for $P p + T t$ is greater than $Q q + R r$.

47. *Secondly.* This attraction must increase by bringing the bodies nearer; for this will increase the difference between $P m$ and $R n$.

48. *Thirdly.* The attraction will increase by increasing the length either of A or of B (the distance $N s$ remaining the same); for by increasing the length of A, which is represented by $p r$ or $q t$, $R r$ is more diminished than $T t$ is. In like manner, by increasing B, whose length is represented by $p q$ or $r t$, we diminish $Q q$ more than $T t$.

49. On the other hand, if the overcharged end of B front the overcharged end of A, their mutual action will be $\frac{F' f' (-P p + Q q + R r - T t)}{F f}$, and A will be repelled, and the repulsion will increase or diminish, by change of distance or magnitude, precisely in the same manner that the attractions did. It is hardly necessary to observe, that all these consequences will result equally from bringing an apparatus similar to that represented in fig. 3. near to another of the same kind; and that they will be various according to the position and the redundancy or deficiency of the two parts of each apparatus.

50. If the body B of fig. 5, is not at liberty to approach toward A, nor to recede from it, and can only turn round its centre B, it will arrange itself in a certain determinate position with respect to that of A. For example, if the centre B (fig. 7.) be placed in the line passing through S and N of the body A, B will arrange itself in the same straight line: for if we forcibly give it another position, such as s B n, N will attract s and repel n, and these actions will concur in putting B into the position s' B n'. S, however, will repel s and attract n; and these forces tend to give the contrary position. But S being more remote than N, the former forces will prevail, and B will take the position s' B n'.

If the centre B be placed somewhere on the line AD, drawn through a certain point of the body NAS, (which will be determined afterwards), at right angles to NAS, the body B will assume the position n' B s', parallel to NAS, but subcontrary. For if we forcibly give it any other position n B s, it is plain that N repels n and attracts s, while S attracts n and repels s. These four forces evidently combine to turn the body round its centre, and cannot balance each other till B assume the position n' B s', where n' is next to S, and s' is next to N.

If the centre of B have any other situation, such as B′, the body will arrange itself in some such position as n' B′ s'. It may be demonstrated, that if B be infinitely small, so that the action of the end of A on each of its extremities may be considered as equal, B will arrange itself in the tangent BT of a curve NB S, such that if we draw NB′, SB′, and from any point T of the tangent draw TE parallel to B′N, and TF parallel to B′S, we shall have B′E to B′F, as the force of S to the force of N. This arrangement of B′ will be still more remarkable and distinct if N be an overcharged sphere, and S an undercharged one, and both be insulated. We must leave it to the reader's reflection to see the changes which will arise from the inequality of the redundancy and deficiency in A or B, or both, and proceed to consider the consequences of the mobility of the electric fluid. These will remove all the difficulty and paradox that appears in some of the foregoing propositions.

51. Let the body A (fig. 4.) contain redundant fluid, and let B be in its natural state, but let the fluid in A be fixed, and that in B perfectly moveable; it is evident that the redundant fluid in A will repel the moveable fluid in B, toward its remote extremity N, and leave it undercharged in S. The fluid will be rarefied in S, and constipated in N. We need only consider the mutual actions of the redundant fluid and redundant matter. It is plain that things are now in the situation described in § 15: A must be attracted by

B, because $f' = m'$, and z is greater than z'. The attractive force is $F' f' \times (z - z')$.

Thus we see that the hypothesis is accommodated to the phenomena in the case in which it appeared to differ so widely from it. Had the fluid been immoveable, the mutual actions would have so balanced each other that no external effects would have appeared. But now the greater vicinity of the redundant matter prevails, A is attracted by B, and, the actions being all mutual, B is attracted by A, and approaches it.

52. We have supposed that the fluid in A is immoveable; but this was for the sake of greater simplicity. Suppose it moveable. Then, as soon as the uniform distribution of the fluid in B is changed, and B becomes undercharged at s, and overcharged at n, there are forces acting on the fluid in A, and tending to change its state of distribution. The redundant matter in S attracts the redundant fluid in A more than the more remote redundant fluid in n repels it, because z' is less than z. This tends to constipate the redundant fluid of A in the nearer parts, and render N more redundant, and S less redundant in fluid than before. It is plain, that this must increase their mutual action, without changing its nature. It can be strictly demonstrated that however small the redundancy in A may be, it can never be rendered deficient in its remote extremity by the action of the unequally disposed fluid in B, if the fluid in B be no more nor less than its natural quantity. It is also plain, that this change in the disposition of the fluid in A must increase the similar change in B. It will be still more rarefied in s, and condensed in n; and this will go on in both till all is in equilibrio. When things are in this state a particle of fluid in B is in equilibrio by the combined action of several forces. The particle B is propelled toward S by the action of the redundant fluid in A. But it is urged toward S by the repulsion of the redundant fluid on the side of n, and also by the attraction of the redundant matter on

the side of *s*; and the repulsion of the redundant fluid in A must be conceived as balancing the united action of those two forces residing in B.

53. Hence we may conclude, that the density of the fluid in B will increase gradually from *s* to *n*. It will be extremely difficult to obtain any more precise idea of its density in the different parts of B, even although we knew the law of action between single particles.

This must depend very much on the form and dimensions of B; for any individual particle sustains the sensible action of all the redundant fluid and redundant matter in it, since we suppose it affected by the more remote fluid in A. All that we can say of it in general is, that the density in the vicinity of *s* is less than the natural density; but in the vicinity of *n* it is greater; and therefore there must be some point between *s* and *n* where the fluid will have its natural density. This point may be called a NEUTRAL point. We do not mean by this that a particle of superficial fluid will neither be attracted nor repelled in this place. This will not always be the case (although it will never be *greatly* otherwise); nor will the variation of the density in the different parts of B be proportional to the force of A on those parts. Some eminent naturalists have been of this opinion; and, having made experiments in which it appeared to be otherwise, they have rejected the whole theory. But a little reflection will convince the mathematician, that the sum of the internal forces which tend to urge a particle of fluid from its place, and which are balanced by the action of A, are *not* proportional to the variations of density, although they increase and decrease together. We shall take the proper opportunity of explaining those experiments; and will also consider some simple, but important cases, where we think the law of distribution of the fluid ascertained with tolerable precision.

If we suppose, on the other hand, that A is undercharged, the redundant matter in A will attract the moveable fluid in

B, and will abstract it from the remote extremity, and crowd it into the adjacent extremity. Moreover, the fluid now becoming redundant in the nearer extremity of B, will act more strongly on the moveable fluid in A than the more remote redundant matter of B; and thus fluid will be propelled toward the remote side of A, which will become now undercharged in its nearer side, and less undercharged in its remote side than if B were taken away. This must increase the inequability of distribution of the fluid in B, and both will be put farther from their natural state; but A will never become overcharged in its remote extremity.

Things being in this state, it is plain that A and B will mutually attract each other in the same manner, and with the same force, as when A was as much overcharged as it is now undercharged.

54. Thus, then, we see how the attraction obtains, whether A be over or undercharged. A fact which Dr. Franklin could never explain to his own satisfaction; nor will it ever be explained consistently with the acknowledged principles and observed laws of mechanics by any person who employs electric atmospheres for this purpose. It is indeed a sufficient objection to the employment of such electric or other atmospheres, that the same extent of attraction and repulsion between the particles of the atmosphere is necessary, as is employed here between the particles of the fluid residing in the body; and therefore they cease to give any explanation, even although their supposed actions were legitimately deduced from their constitution. This is by no means the case. Let any person examine seriously the *modus operandi* of the electric atmospheres employed by Lord Mahon (the only person who has written mathematically on the subject), and he will see, that the whole is nothing but figurative language, without any distinct perception of what is meant by these atmospheres, as distinct from the fluid moveable in the conducting bodies, or any perception how the unequal density of these atmospheres protrudes the fluid

along the conductor. Besides, it is well known that a conducting wire becomes positive at one end, and negative at the other, by the mere vicinity of an overcharged and undercharged body, and this in an instant, although it be surrounded with sealing-wax, or other non-conductors, to any thickness: in this case there can be no atmospheres to operate on the included fluid. To this we may add Dr. Franklin's judicious experiment of whirling an electrified ball many times round his head, with great rapidity, by means of a silk line, without any sensible diminution of its electricity. It is not conceivable that an electric atmosphere could remain attached to the ball; nor could it be instantaneously formed round the ball, in every point of its motion, so as to be operative the moment he stopped it and tried it; for this would have exhausted or greatly diminished the electricity of the ball; whereas that sagacious philosopher affirms (and any person will find it true), that when the air is dry, he did not observe the electricity more diminished than that of another ball which remained all the while in the same place.

55. Let the overcharged body A (fig. 6.) be brought near the ends of two oblong conductors B and C in their natural state, and lying parallel to each other; the fluid will be propelled toward their remote ends N, *n*, where it will be condensed, while it will be rarefied in the ends S and *s*, adjacent to A. Both will be attracted by A, and will attract it. But the redundant fluid in NB will repel the redundant fluid in *n* C; and the redundant matter in SB will repel the redundant matter in *s* C. For this reason the bodies B and C will repel each other, and will separate; but SB attracts *n* C, and NB attracts *s* C; and on this account the bodies should approach: but the distances of the attracting parts being greater than those of the repelling parts, the repulsions must prevail, and the bodies must really separate.

It is equally clear that the very same *sensible* appearance will result from bringing an undercharged body near the

ends of B and C, although the internal motions are just the opposite to the former.

If another body D, electrified in the same way with A, be brought near the opposite ends of B and C, it will prevent or diminish the internal motions, and it should therefore prevent or diminish the external effects.

If another conducting body E be brought near to the end *s* of C that fronts A, it will be affected as C is, and the end *f* will repel *s*; but if it be brought near the remote end, as is the case with the body F, it will attract this remote end. As the body A, containing more or less than its natural share of electric fluid, affects every other body, while they do not (when out of its neighbourhood) affect each other, it is usually said to be the electrified body, and the others are said to be electrified by it; and since these bodies, when perfect conductors, cannot retain their power of exhibiting electrical appearances (see § 17), it will be convenient to distinguish this last electrical state by a particular name. We shall call it ELECTRICITY BY POSITION, or INDUCED ELECTRICITY. It is induced by position with regard to the permanently electrical body.

56. We have supposed, in these last propositions, that the fluid was perfectly moveable in B, and, at last also, in A: but let us examine the consequences of some obstruction to this motion. Without entering into a minute enquiry on this head, we may state the obstruction as uniform, and such that a certain small force is necessary for causing a particle of fluid to get through between two particles of the common matter, just as we conceive to happen in tenacious bodies of uniform texture (see § 18.)

It is evident that when an overcharged body A (fig. 5.) is brought near such an imperfect conductor B, the fluid cannot be so copiously propelled to the remote extremity *n*. We may conceive the state of distribution by taking a constant quantity from the intensities of the force of A at every point of B. This circumstance alone shews us, that there

will not be so unequable a distribution of the fluid, and therefore *there will not be such a strong attraction between imperfect as between perfect conductors.* But besides this, we see that an incomparably longer time must elapse before things come to a state of equilibrium. Each particle of fluid employs time to overcome the obstacle to its motion, and it cannot advance till after the succeeding ones, each escaping in its turn, have again come up with the foremost. An important consequence results from this. The neutral point, where the fluid is of the natural density, will not be so far from the other body as it would have been without these obstructions; and this point will be a considerable while of advancing along the imperfect conductor. At the first approach of the overcharged electric, the near extremity of the imperfect conductor becomes a little undercharged, and the neutral point advances from the very extremity a small way, the displaced fluid being crowded a little before it, and giving way by degrees as its foremost particles get past the obstructions. The motion forward takes place over a considerable extent at the very first; namely, in that part of the conductor where the propelling power of the neighbouring electric is just able to push a particle over the obstruction. As the propulsion goes on, the neutral point must gradually advance, and at last reach a certain distance, determined by the degree of the obstruction. It is plain, that the final accumulation at the remote end of the imperfect conductor will be less than in a perfect conductor, and the neutral point will be nearer to the other end.

57. There is another remarkable consequence of the obstruction. It must always happen that, at the beginning of the action, the greatest constipation will not be towards the remote extremity, but in a place much nearer to the disturbing cause. Beyond this, the constipation will diminish. As time elapses during this operation, this constipated fluid acts on the fluid beyond it by repulsion, and may do this with sufficient force to displace some of it, and render a part of

the imperfect conductor deficient, with a small constipation beyond it. This may, in like manner, produce a rarefaction farther on, followed by another condensation; and this may be frequently repeated when the obstruction is very great, and the repulsion of the overcharged body very great also. This can be strictly demonstrated in some very simple cases, but the demonstration is very tedious: As the result, however, is of the first importance in the theory of electricity, and serves to explain some of the most abstruse phenomena, we wish the reader to have some stronger ground of confidence than the above bare assertion. He may observe similar effects of causes precisely similar. If we dip the end of a flat ruler into water, and if, after allowing the water to become perfectly still, we move the ruler gently along in a direction perpendicular to the face, we shall observe a single wave heap up before the ruler, and keep before it, all the rest of the water before it remaining still: but if we do the same thing in a vessel of clammy fluid, especially if the clammy part is swimming on the surface of a more perfect fluid, like a cream, we shall observe a series of such waves to curl up before the ruler, and form before it in succession; and if we have previously spotted the surface of the cream, we shall see that it is not the same individual waves that are pushed before the ruler, but that they are successively formed out of different parts of the surface, and that the particles which, at one time, form the summit of a wave, are, immediately after, at the bottom, &c. In like manner, when a cannon is fired in clear air, at no great distance, we hear a single snap; but, in a thick fog, we hear the snap both preceded and followed by a quivering noise, resembling the rushing of a fluttering wind, which lasts perhaps half a second. A slight reflection on these facts will shew that they are necessary results of the mechanical laws of such obstruction.

The consequence of this mode of action must be, that an imperfect conductor may have more than one neutral point,

and more than one overcharged and undercharged portion, so that its action on distant bodies may be extremely various. The formula of § 28. was accommodated to this case, and will be found to have very curious results. Another body may be placed in the direction of the axis, and will be attracted at one distance, repelled when this distance is increased, and again attracted when at a still greater distance, &c. &c.

Suppose the obstruction not to be considerable: The immediate operation of the neighbouring overcharged body will be the production of an undercharged part in the adjoining extremity, an overcharged part beyond this, an undercharged portion farther on, &c. In a little while these will shift along the conductor; one after another will disappear at the farther end, and the body will have at last but one neutral point. A greater obstruction will leave the body, finally, with more than one neutral point, and their ultimate number will be greater in proportion as the obstruction to the fluid's motion is supposed greater.

58. Now, let the overcharged body, the cause of this unequal distribution, be removed. We have seen § 17. that when a body contains its natural quantity of fluid, but unequally distributed, there is a force acting on every particle, and tending to restore the original equable distribution; and that such a force remains as long as there is any inequality in this respect. If, therefore, there be no obstruction, the uniform distribution will take place immediately; for it is well known, that the speed with which electricity is propagated is immense. The elasticity, or the attractive and repulsive forces, must be very great indeed when compared with any that we know, except, perhaps, the force which impels the particles of light. The electricity, therefore, of a perfect conductor, that is, its power of acting on other bodies in the same way that an original electric acts on them, must be quite momentary, and cease as soon as the inducing cause is removed. The conductor is electrical merely in

consequence of its position. Hence the propriety of our denominations. Nothing material is supposed in this theory to be communicated from the overcharged body: Nay, this theory teaches, that the sensible electricity of the overcharged body is augmented in some respects; for it becomes more overcharged in the part nearest to the conductor. Indeed it becomes less overcharged on the other end, and will act less forcibly on that side than if the conductor were away. It may be remarked here (it should have been mentioned in § 55.), that when F is presented in the manner shewn in fig. 6. the body B becomes more strongly overcharged at the end remote from A, and more strongly undercharged at the end next to A, than when F is away. The contrary *may* happen, by presenting a body in the manner of E. We wish these particulars to be kept in mind. In the mean time, all these circumstances are necessary consequences of the supposition, that nothing is communicated from A to B or C. The electricity induced on perfect conductors is momentary, requiring the continual presence of a body that is electrified in some way or other.

But the case is quite otherwise in imperfect conductors. When the overcharged, or otherwise electrical body A is removed, the forces which tend to restore the uniform distribution of the fluid immediately operate, and must restore it in part. They cannot, however, do it completely: For when the force which urges any particle from an overcharged to an undercharged part is just in equilibrio with the obstruction, it will remain, just as a number of grains of small shot may lie, uniformly mixed with a mass of clammy fluid, or, as such fluids retain heavy mud, in a state of equable or inequable diffusion. If the resistance arise merely from the inertia of the tangible matter, there is no force so small but it will in time restore the uniform distribution. But this cannot be the case in solid bodies. Their particles exert lateral forces, by which they maintain themselves in particular situations; these must be overcome by *superior* forces.

We should therefore expect, that imperfect conductors will retain part of their inequable constitution; and, *in consequence of this*, their power of affecting other bodies like electrics; that is, their ELECTRICITY. For we must observe (having neglected to do it in the beginning), that the term *electricity* is as often used to express this power of producing electrical phenomena as it is used for expressing a substance supposed to be the original cause of all these appearances. It is necessary to keep this distinction in mind; because there are many phenomena which clearly indicate the transference of this cause, and they must not be confounded with others, where the exhibition of electric phenomena is evidently propagated to a distance. We must not always suppose, that when the electric appearances are exhibited in an instant at the far end of a wire $4\frac{1}{2}$ miles long, the same numerical particles of the electric fluid have moved over this space. We must distinguish those cases where this must be granted from those in which it certainly has not happened. Of these there are innumerable instances.

59. We have now to observe, that by this theory the single circumstance of perfect and imperfect conducting power is sufficient for establishing the whole difference between idio-electrics and non-electrics. The idio-electrics are susceptible of excitation in various ways, and retain their electricity; and this may be done in any part of them without affecting the rest in any remarkable degree. This cannot be done in perfect conductors, plainly *because they are perfect conductors*. Any inequality of distribution of the electric fluid, which is all that is necessary for rendering them electric, is immediately destroyed by its uniform diffusion. We can have no direct proof of their incapability of excitation; but if they can be excited, they cannot shew it. We doubt, however, their excitability; because the appearances in the excitation of electrics seem to indicate, that opposite states of two bodies are necessary previous to the appearance of electricity. This is impossible in perfect conductors. By

this theory, therefore, perfect conductors are necessarily non-electrics; and non-conductors are necessarily (if excitable) idio-electrics.

With respect to the particular phenomena which may be expected on the removal of the original electric; it may just be remarked, that the electric appearances of the imperfect conductor will go off in the contrary order to that of their indication. The accumulation and deficiency will diminish gradually, and the neutral point or points will gradually approach the end which had fronted the original electric. The imperfect conductor will be finally left with one or more neutral points, according to the magnitude of the obstructions, and the force which had been employed in its electrification: And their final state will be so much the more inequable, and consequently they will retain so much the greater electric powers, as they are less perfect conductors.

60. The last observation which we shall make on this head at present is, that whether electrified by induction, or by friction, or most other modes of excitation, the electrification will be nearly superficial in bodies which conduct very imperfectly; and bodies which are altogether impervious (if there be any such) must have the accumulation or deficiency altogether at their surface. If a glass globe be such a body, it will hardly be possible to electrify it to any depth; and all that we can expect is alternate strata of overcharged and undercharged glass. If these strata are once formed, they tend greatly to make the body retain its superficial electricity. A superficial stratum of redundant fluid, tending, by the mutual repulsion of its particles, to escape, is retained by the stratum of redundant matter immediately below it: And the almost insuperable obstruction prevents the fluid of the stratum beyond this from coming up to supply the vacancy. If we can fall on any contrivance to produce such deficient strata within the glass, we shall make it much more retentive, and capable of holding fast a much greater quantity. We have already mentioned something of this in

§ 14. and we recommend the case to the attentive consideration of the reader.

61. Thus have we given a sketch of the leading doctrines of this elegant theory of Mr. Æpinus, all legitimately deduced from the circumstances assumed in the hypothesis concerning the *mechanical* properties of that substance which he calls *the electric fluid*. Let us now see with what success this hypothesis may be applied to account for the phenomena. It would have been more philosophical to have arranged the phenomena, and from the comparison to have deduced the hypothesis. But this would have required much more room than can be afforded in a work like ours.

We presume, that many of our readers, namely, all such as are already conversant with electrical phenomena and with electric experiments, have seen, as we went along, the perfect agreement of the hypothesis with the various phenomena of attraction and repulsion, and all those which are usually classed under the name of electric atmospheres: and we are confident, that when they compare the consequences that should necessarily result from such a fluid with the legitimate consequences of the mechanical action of elastic atmospheres, they will acknowledge the great superiority of this hypothesis in point of simplicity, perspicuity, and analogy with other general operations of nature. To such readers it would not be necessary to state any farther comparison; but there are many who have not yet formed any distinct *systematic* view of the appearances called *electrical*. We do not know any way of giving such a view of them as by means of this hypothesis; and we may venture to say, that it will enable the student of Nature to class them all, with hardly a single exception. After which, the hypothesis may be thrown aside by the fastidious philosopher; and the useful classification, and general laws of the electric phenomena, will remain ready foundations for a more perfect theory. For the sake of such readers, therefore, we shall

take a short review of those general appearances which are accompanied by attractions and repulsions, and compare them with this Æpinian theory.

We shall not at present consider the various modes of excitation, although this theory also affords much instruction on the subject, but confine ourselves entirely to the facts which are most immediately dependent on it, and should be employed to support or overturn it; and we shall suppose the reader acquainted with most parts of the common apparatus; such as electrometers, insulation, &c. We also presume that he knows, that when a small pith-ball has been electrified by touching a piece of glass which has been excited by rubbing with dry flannel, it will repel another body so electrified; and that balls, which have received their electricity in this manner from sealing-wax excited by the same rubber, also repel each other; but that balls, thus electrified by glass, attract those which are electrified by sealing-wax.

The following simple apparatus will serve for all the experiments which are necessary for establishing the theory:

62.—1. Two slender glass rods A (fig. 8.), having a brass ball B at the end, about a quarter of an inch in diameter, suspending a very small and delicate pith-ball electrometer C.

2. Some electrometers (fig. 9.), consisting of two pieces of rush pith, about four inches long, nicely suspended, and hanging parallel, and almost in contact with each other. It is proper to have them as smooth as possible, and neatly rounded at the ends, to prevent unnecessary dissipation.

3. Some pith-ball electrometers (fig. 10.), whose threads are of silk, about four inches long, and some with flaxen threads moistened with a solution of some deliquescent salt, that they may be always in a good conducting state.

4. Several brass conductors (fig. 11.), each supported on an insulating stalk and foot. They should be about an inch and half or two inches long, and about three-fourths of an

inch in diameter, with round ends, and well polished, to prevent all dissipation. The foot must be so narrow as to allow them to touch each other at the ends.

5. Two balls (fig. 12.), one of glass, and the other of glass coated with sealing wax, each furnished with an insulating handle, the other end of which may be occasionally stuck into a foot, or into the side of a block of wood, which can be slid up or down on a wooden pillar, and fixed at any height. These balls should be about three inches in diameter. They must be excited by rubbing with dry warm flannel.

6. Some little pieces of gilt card (fig. 13.), about two inches long, half an inch broad, and rounded at the ends, and made as smooth as possible. Each must have a dimple struck in the middle with a polished blunt point, so that it will traverse freely like a mariner's needle when set on a glass point, rounded in the flame of a lamp. More artificial needles may be made of some light wood, having small cork balls at the ends, all gilt and polished, and turning, in like manner, on glass stalks: also some similar needles made of sealing wax, one end of each being black, and the other red.

The mechanical phenomena of electricity may be expressed in a few simple propositions. The most general fact that we know, and from which all the rest may be deduced, is the following:

If any body A is electrified, by any means whatever, and if another body B is brought into its neighbourhood, the last becomes electrical by position.

63. Set the brass conductors in a row, touching each other, as represented in fig. 11. by A, B, C; and let a pith ball electrometer, having silk threads, be set near one end of the conductors. Excite one of the above mentioned globes by rubbing it withdry flannel. When this is brought near the end of the conductor, the pith-ball will approach the other end. But the globe must not be brought so near as to cause

the pith-ball to strike against the other end. On removing the globe, the pith-ball will move off and hang perpendicularly. The same effect is produced by both globes.

Thus the mere vicinity of the electric renders the conductor electric, and the electricity ceases on removing the globe. This is perfectly conformable to the theory, whether we suppose the fluid to be made redundant or deficient at the remote end of the conductor. If one should ascribe the approach of the pith-ball to the immediate action of the globe, it is sufficient to observe, that if the ball be suspended near the *side* of the conductor, it will approach the conductor, shewing that it is affected by the conductor, and not by the globe.

Let the globe be held in the position D (fig. 12.) about six inches from the conductor, and a little above the line of its axis. Take the glass rod (fig. 8.), and bring its knob into contact with the under side of the remote end *c* of the conductor. The balls of the electrometer will separate, shewing that they are electrified in the same manner, and repel each other. Slide the brass knob along the under side of the conductors, quite to the end *a*. The balls will gradually collapse as the knob approaches a point near the middle of the conductors, where they will hang parallel. Passing this point, they will again separate, and most of all when the knob is *a*. In this situation they will deviate toward the globe, and will be directed straight toward it, if it be held too near, or in the direction of the axis. This would disturb the experiment, and must be avoided. These phenomena are conformable to the account given of the disposition of the fluid in the conductor. The electrometer may be considered as making a part of the conductor; and when its threads hang parallel, it is in its natural state, having its fluid of its natural density. This, however, cannot be strictly true, according to the theory; because the balls of the electrometer must be considered as more remote from the electric, and their electrical state must correspond to a point

of the conductor more remote than that where the knob of the electrometer touches it. This will be more remarkably the case as the threads are longer. Accordingly, an electrometer with very long threads will never collapse. The place of the neutral point cannot be accurately ascertained in this way. Lord Mahon imagined, that its situation B was determined (in his experiments with a long conductor) to be such, that D *c* was harmonically divided in B and *a*; and he finds this to be agreeable to the result of an electric atmosphere whose density is inversely proportional to the square of the distance. But we cannot deduce this from his narration of the experiment. He gives no reason for his selection of the point D, nor tells us the form and dimensions of the electric employed, nor takes into account the action of the fluid in the long conductor. It is evident that no computation can be instituted, even on his lordship's principles, till all this be done. We have always found, that the neutral point was farther from the electric, in proportion as the conductor was smaller, and when the electricity was stronger; and that the differences in this respect were so very considerable, that no dependence could be had on this experiment for determining the law of action. It should be so, both according to Lord Mahon's and Mr Æpinus's theory. But to proceed with our examination:

Having touched the end *c* of the conductor with the knob of the electrometer, bring it away. The balls will continue to repel each other, and they are attracted by any body that is in its natural state. Touch the same end with the knob of the other electrometer, and bring it also away; the balls of the two electrometers will be found to repel each other: but if one has touched the conductor at *c*, and the other has touched it at *a*, the electrometers will strongly attract each other. All this is quite conformable to the theory. If the fluid has been compressed at *c*, and therefore the balls of that electrometer are overcharged, they must repel each other, and repel any other body electrified in the same way.

They must attract and be attracted by any natural body. But the balls of the other electrometer having touched the conductor at *a*, must be undercharged, and the redundant fluid of the one must attract the redundant matter of the other.

If the conductor has been electrified by the vicinity of excited glass, the electrometer which touched it in the remote end *c*, will be repelled by a piece of excited glass, but attracted by excited sealing-wax. The electrometer which touched the conductor in *a* will be attracted by excited glass, and repelled by excited sealing-wax. The contrary will be observed if the conductor has had its electricity induced on it by the vicinity of the globe covered with sealing-wax. This is a complete proof that Mr. Dufay's doctrine of *vitreous* and *resinous* electricity is unfounded. Both kinds of electricity are produced in a conducting body, without any material communication, by mere juxta-position to a body possessed of either the vitreous or the resinous electricity.

64. We have not yet mentioned any reasons which indicate which end of the conductor is electrical by the redundancy of electric fluid, nor is the reader prepared for seeing their force. It is generally believed, that the remote end of a conductor which is electrified by glass, excited by rubbing it with flannel or amalgamated leather, is electrical by redundancy. No difference has been observed in the attractions and repulsions. But there are other marks of distinction which are constant, and undoubtedly arise from a difference in the mode of action of those mechanical forces. If, while the excited glass globe remains at D, a glass mirror, foiled as usual with tin-leaf, be made to touch the remote end of the conductor, and slowly drawn transversely, so that the conductor draws a line as it were across it—this mirror being laid down with the foiled side undermost, the dust, which settles on it in the course of a day or two, will be chiefly collected along this line, somewhat in the form of the fibres of a feather. But if the conductor was render-

ed electrical by the globe covered with sealing-wax, the dust will be collected along this line in little spots like a row of beads. The appearances will be reversed if the mirror has been passed across the end of the conductor which is nearest to the excited electric. In short, in whatever way the drawing point has been electrified, if it repel a ball which has touched excited glass, the line will be feathered; but if it attract such a ball, the line will be spotted. There are many ways of making this appearance much more remarkable than this; but we have mentioned it on this occasion, because the circumstances which occasion the difference, whatever it is, are the most simple possible. Nothing is communicated; and therefore the effect must arise from the unnatural state of a substance or power residing in the body. If it be a substance *sui generis*, the electric action must arise from a different distribution of this substance; from a redundancy and deficiency of it in the different portions of the conductor. Without pretending as yet to say which is redundant, we shall suppose, with Dr Franklin, that the electricity of excited glass is so; and we shall use the words *redundant* and *positive* to distinguish this electricity from the other. This is merely that we may, on many occasions, considerably abbreviate language.

The different electrical states of the different portions of the conductor may be seen in another way, which is perhaps more simple and unexceptionable than that already narrated. While the globe remains at D, take the two extreme pieces A and C aside; or, if only two pieces have been used, draw the remote piece farther away. Now remove the excited globe. When we examine A separately, we shall find it wholly negative, or undercharged, strongly repelling a ball electrified by sealing-wax, and attracting a ball electrified by glass. The other piece C exhibits positive electricity, attracting and repelling what A repelled and attracted. If only three pieces of the conductor have been employed, the middle piece B is generally positive; but this in a very faint degree.

If all the pieces be again joined, they are void of electricity. If, instead of such conductors, a row of metal balls, suspended by silk lines, are employed, one of them may generally be found without any sensible electricity, when separated from the rest, having been the neutral part of the row while united.

These very simple facts shew, as completely as can be wished, that if the electric phenomena depend on a fluid moveable in the pores of the body, the constitution given it by Mr Æpinus is adequate to the explanation. We may now venture to assert, that every other phenomenon of attraction and repulsion will be found in exact conformity with the legitimate consequences of this constitution of the electric fluid.

65. That nothing is communicated from the electric will appear still more forcibly by the following experiment: Let a conductor be rendered electrical in the way now described, and touch either extremity of it with the little electrometer, and observe attentively the divergency of its threads. Now approach its more remote extremity with another conducting body, such as a single piece of those conductors, it will be rendered electrical; as may be discovered by a delicate electrometer. Observe carefully whether the electrometer in contact with the first conductor be affected:—it will generally be found to spread its threads wider. It will *certainly* be thus affected if the other conductor be very long and bulky, or touched by the hand; or if, instead of this second conductor, we approach the first with the extended palm of the hand. As the second conductor was rendered electrical, so, undoubtedly, is the hand also: and its electrification has not deprived the first conductor of any of its electric power, but, on the contrary, has increased it. And this augmentation of its power is equally sensible at both ends: For an electrometer at the other end will also diverge more when the hand is brought near the remote end. This theory explains this in the most satisfactory manner. The

first conductor renders the second electric, by propelling its fluid to a greater distance. The second conductor now acts on the fluid that is moveable in the first, and causes a greater accumulation in its end which is farthest from the electric; that is, renders it more electric.

66. Suppose that instead of employing an excited globe of glass, we had made use of a conducting body, slightly overcharged. Thus if we employ the conductor A, overcharged, to induce electricity on C; this will produce the same general effect on our set of conductors. But if we have previously examined the force of the redundant body, by suspending a pith-ball near it, and observing its deviation from the perpendicular, we may sometimes be led to think, that it has imparted something to the other body. For if the other body and the pith-ball be on opposite sides of the redundant body, the pith-ball will fall a little; indicating a diminution of electric force. But this *should happen* according to the theory; for it was shewn, in § 52. that the constipation in the remote end of the overcharged body will be diminished, and along with this, its action on the pith-ball. We should find the electricity of the other end, next the conductor, increased, could we discover an easy way of examining it; but an electrometer applied there will be too much affected by the conductor.

The same conclusions may be drawn from the following facts: Hang up a rush-pith electrometer. Approach it below with a body slightly electrified. The legs of the electrometer immediately diverge, though attracted by the electrified body. Hold the hand above the electrometer, and they will diverge still more; touch the top of it, and they spread yet farther. Hold the electrified body (very weakly electrified) above the electrometer, so that its legs may diverge a little. Hold the hand above the electrified body; the legs of the electrometer will come nearer each other.

These appearances are observed whether the electric be positive or negative. We need not take up time in explaining this by the theory, its agreement is so obvious.

Lastly, on this head, if, in place of a fixed conductor, we use one of the needles of gilt card, set on its pivot, and if we then approach it with another conducting body, in the manner represented by E and C of fig. 6. we shall observe that end of the needle to avoid the other body; but if we bring them together, in the manner represented by F and B, they will attract each other. The attraction will be greater when the body F is long; and most of all when it communicates with the ground. These phenomena are therefore in perfect conformity with the theory; but it may sometimes happen that E will attract the end of C that is nearest to A, and E will be electrified positively if A be positive. This seems inconsistent with the theory: and, accordingly, it has been adduced by Volta against Lord Mahon's account of the electrical state of a conductor in a situation similar to that of C. But the theory of Æpinus shews the possibility of this case. When E is very long, or when it is held in the hand, it is rendered *much more* undercharged than the adjacent part of C; and the fluid in the remoter, but not much remoter, part of C is strongly attracted by the copious redundant matter in the near end of E, and is brought back again, and passes over into E, in the way to be described immediately. The case is rare, and it will not happen at any considerable distance from the neutral point of C. If, indeed, E touch the near end of C before A is brought near, the approach of A will cause fluid to pass into E immediately, and C will be left undercharged on the whole.

The reader, who is at all conversant with electrical experiments, will be sensible, that these experiments are delicate, requiring the greatest dryness of air, and every attention to prevent the dissipation of electricity during the performance. This, by changing the state of the conductors and electrometers, will frequently occasion irregularities. The electrometers are most apt to change in this respect, it being scarcely possible to make them perfectly smooth and free from sharp angles. It may therefore happen, that when the

conductors have affected them *for some time* by the action of the disturbing electric, the removal of this electric will not cause the electrometers to hang perpendicular; they will often be attracted by the conductors, and often repelled; but the intelligent experimenter, aware of these circumstances, will know what allowances to make.

67. The theory obtains a still more complete support from a comparison with similar experiments made with imperfect conductors. If, in place of the series A, B, C, (fig. 11.) of metalline conductors, we employ cylinders of glass or sealing wax, or even dry wood or marble, and electrometers with silk threads in place of the rush-pith electrometers, we shall find all the appearances to be such as the theory enables us to predict. If, for example, we use a single cylinder A of glass, we shall find that the neighbourhood of the electric D (fig. 12.) scarcely induces any electricity on A. The electrometer will hardly exhibit the smallest attraction, and its motions will be almost entirely such as arise from the immediate influence of the electric body D. A cylinder of very dry wood will be more affected by the electric D; and a circumstance of theoretical importance is very distinctly observed, namely, the gradual shifting of the neutral point. It will be found to advance along the cylinder for a very long while, when every circumstance is very favourable, the air very dry, and the wood almost a non-conductor; and its final situation will be found much nearer to the electric than in the brass conductor. Several instructive experiments of this kind may be found in a treatise published in 1783 by Dr. Thomas Milner at Maidstone in Kent, entitled, "Experiments and Observations on Electricity." The author does not profess to advance any new doctrines, but only to exhibit experiments scientifically arranged for forming a system. He supports the Franklinian system as it was generally understood at that time; but is much embarrassed for the explanation of the repulsion of negative electrics. The Æpinian correction of this theory did not offer itself to his mind.

68. We need not go over the same ground again with imperfect conductors. It is well known that such bodies are more weakly attracted and repelled; that the balls of an electrometer with linen threads diverge vastly more when an electrified body is held below it, than if the threads are silken; that such electrometers frequently exhibit very capricious appearances from the slow but real progress of the electricity along the threads. These anomalies will be better understood when we explain the dissipation of electricity along imperfect conductors.

69. A very essential deduction from the theory is, that the electricity induced on an imperfect conductor must have some permanency. This is fully confirmed by experiment. But the remarkable instances of this particular cannot be produced till we be better acquainted with the methods of producing great accumulations of fluid. It is enough to observe at present, that a permanent electricity may always be observed at the junction of the conductors with their insulating stalks. The brass conductor A ceases to be electric as soon as the excited globe is removed; but the very top of the glass stalk on which it is supported will sensibly affect a delicate electrometer for a long while after. The following pretty experiment shews this permanency very distinctly. Set one of the sealing-wax needles on its pivot, and place it between two insulated metal spheres of considerable size, at such a distance from both as not to receive a spark. Electrify these balls moderately, one of them positively, and the other negatively, and keep them thus electrified for some hours by renewing their electrification. The needle quickly arranges itself in the line joining the two spheres, just as a magnetic needle will do when placed between two magnets whose dissimilar poles front each other. Any gentle force will derange the needle; but it will vibrate like a magnetic needle, and finally settle in its former position. When this has been continued some time, that end of the needle which pointed to the positive globe will

be found negative, and the other will be found positive, if examined with an electroscope. And now, if the two globes be removed, this little needle will remain electrical for entire days in dry frosty weather, and its ends will approach any body that is brought near it (taking care not to come too close); and the end which pointed to the positive globe will avoid a piece of rubbed sealing wax, but will approach a piece of rubbed glass; but the other end will be affected in the opposite way. In short, it proves an electric needle with a positive and negative pole.

If two small insulated balls are moderately electrified and placed about six inches asunder, this needle, when carried round them, will arrange itself exactly as a magnetic needle does when carried round a magnet of the same length. If the same trial be made with the needle of gilt card, it will arrange itself in the same manner that a soft iron needle arranges itself near a magnet, but either end will turn indifferently to either globe.

If a thin brass plate, coated with red sealing wax, be set on the positive and negative globes, and we sprinkle (from a considerable height) a fine powder of black sealing wax, and then pat the plate gently with a glass rod so as to agitate it a little, the particles of wax powder will gradually arrange themselves into curve lines, diverging from the point over one of the globes, and converging to the point over the other, precisely like the curves formed by iron-filings sprinkled on a paper held over a magnet. Each little rag of wax becomes electrical by position, acquires two poles, and the positive pole of one attracts the negative pole of another; and they adhere in a certain determinate position, nearly a tangent to the curve, which was mentioned in § 50. and indicates the law of magnetic action. When in this state, if a hot brick be held over the plate till the wax soften a little, the particles of black wax will adhere to the red coating, and give us a permanent specimen of the action.

70. It is well known that liquid sealing wax is a conductor. The writer of this article filled a glass tube with powdered sealing wax, and melted it, and then exposed it, in its melted state, to the influence of a positive and a negative globe, hoping to make a powerful and permanent electric needle, which should have two poles, and exhibit a set of phenomena resembling those of magnetism. Accordingly he, in some measure, succeeded, by keeping the globes continually electrified for several hours, till the wax was quite cold. It had two distinct poles, and preserved this property, *even though plunged in water, and while immersed in the water;* but he was greatly disappointed as to the degree of its electricity. It just affected a sensible electrometer at the distance of six inches from either pole. It was considerably stronger than if it had not been melted during the impregnation, but by no means in the degree that he expected. It retained some electricity for about six weeks, although lying neglected among conducting bodies. After its power seemed quite extinct, he was melting it again in order to renew it. Some light fibrous things chanced to be near it. While it was softening, it became very sensibly electrical, causing these fibres to bend towards it, and even to cling to the tube. We shall see by and bye, that he was mistaken in expecting more remarkable appearances, and that the theory, when properly applied, does not promise them. Having thus established (as we think) this theory on sufficient foundations for making it a very perspicuous way of explaining the phenomena of induced electricity, we proceed to compare it with the second general fact in electricity.

71.—Prop. II. When an insulated body B is brought very near an electrified body A, a spark is observed to pass between them, accompanied with a noise (which we shall call the electric Snap), and B is now electrified permanently, and the electricity of A is diminished.

Although this be one of the most familiar facts in electricity, it will be proper to consider its attending circumstances in a way that connects it with what we have now learned concerning electricity by position.

Let the insulated body A (fig. 14.) be furnished with a cork-ball, hanging by a silk thread from a glass stalk connected with A; let B be fitted up in the same manner; let A be electrified weakly, and its degree of electricity be estimated by the inclination of the ball *towards* A: since B is not electrified, its electrometer will hang perpendicular; but when it approaches A (keeping the electrometers on the remote sides of both), its electrometer will approach it, and the electrometer of A will gradually approach the perpendicular. When the bodies are brought very near, a spark is seen between them; and, at that instant, the electrometer of B comes much nearer to it, and that of A drops farther from it. If they be now separated, their electrometers will retain their new positions with very little change, and B will now manifest the same kind of electricity with A.

Such is the appearance when A has been but weakly electrified. Bringing B near A, the fluid in B is drawn to the remote side, if A be overcharged, or drawn to the side nearest to A, if A has been undercharged. B acts on its electrometer in consequence of the change made in the disposition of its fluid. The electrometer is attracted. In the mean time, the change made in the disposition of the fluid in B affects the moveable fluid in A. If A was overcharged, the adjacent side of B becomes undercharged, and its redundant matter, attracting the fluid in A, condenses it in the adjacent side, abstracting part of the redundant fluid from that side which is next to the pith-ball. Then the joint action of the whole redundant fluid in A on the pith-ball is diminished.

As there is now an attraction in the redundant fluid in A for the redundant matter on the adjacent side of B, it is

reasonable to suppose, that when this attraction, joined to the repulsion of the redundant fluid behind it, is able to overcome the attraction which connects it with the superficial particles of the matter, it will then escape and fly into B; but this will not happen gradually, but at once, as soon as the expelling force has arisen to a very considerable intensity. We cannot say what is the precise augmentation that is necessary; but we can clearly see, that however great the attraction for the adjoining particles may be, while the particle is surrounded by them on *all* sides, it will yield to the smallest inequality of force, because the particles before it attract as much as those behind it; but when it is just about to quit the last or superficial particles of A, a much greater force is now necessary. It can be strictly demonstrated, that when the mutual tendency is inversely as the square of the distance, the action of a particle placed immediately without a sphere of such matter is double of its action when situated in the very surface. A *saltus* of this kind must obtain whatever be the law of electric attraction. We shall see other causes also which should prevent the escape of redundant fluid, and also its admission, till the impelling force is increased in a certain abrupt degree.

72. These observations must suffice at present to explain the desultory nature of this transference, if there be really a transference. That this has happened, may be confidently inferred from the sudden diminution of the electricity of A, indicated by the sudden fall of its electrometer; but it is more expressly established, that there has been a transference by the change produced on B. It is now permanently electrified, and its electricity is of the same kind with that of A, positive or negative according as A is positive or negative. And now we are enabled to explain the third general fact in electricity.

73.—Prop III. When a body has imparted electricity to another, it constantly repels it, unless that other has af-

terwards imparted all its electricity to other bodies. This fact, from which there is no exception, is an immediate consequence of the theory. Before the transference supposed by it, B was in its natural state; after the transference, both bodies contain redundant fluid, or redundant matter; therefore they must mutually repel.

We may now take another form of the experiment, which will be much more convincing and instructive. Let A be electrified positively, or by redundancy, and let its electrometer be attached to it by a conducting stalk, and have a flaxen thread; let this be the case also with the electrometer of B; then the appearances should happen in the following order: When A is made to approach B, the electrometer of B must gradually rise, diverging *from* B; because the fluid condensed on the side remote from A, and in the electrometer, will act more strongly on it than the deserted matter on the other side of B; and when the sudden transference is made, and B is wholly overcharged, its electrometer will immediately rise much higher, and must remain at that height, nearly, when A is removed. On the other hand, the electrometer attached to the remote side of A must descend, by reason of the change made in the disposition of the fluid in A by the induced electrical state of B; and when a considerable portion of the redundant fluid in A passes into B, the electrometer of A must suddenly sink much lower, and remain in that state when B is removed.

Many circumstances of this phenomenon corroborate our belief of a real transference of matter. The cause of electric action resided formerly in A alone; it now resides also in B. The larger that B is, the greater is the diminution of A's electric power, and the smaller is the power acquired by B. It perfectly resembles, in this respect, the communication of saltness, sweetness, &c. by mixing a solution of salt or sugar with different quantities of water; and the evidence of a transference of a substance, the cause of electric attractions and repulsions, is at least as cogent as the

evidence of the transference of heat, when we mix hot water with a quantity of cold, or when a hot solid body is applied to the side of a cold one. We also see so many chemical and other changes produced by this communication of electricity, that we can hardly refuse admitting that *some material substance* passes from one body to another, and, in its new situation, exerts its attractions and repulsions, and produces all their effects.

We may deduce the following corollaries; all of which are exactly conformable to the phenomena, serving still more to confirm the justness of the theory.

74. *First*, A certain quantity of what possesses these powers of attraction and repulsion is necessary for giving a determined vivacity to the appearances. Another spark must pass between the bodies, *only if they be brought still nearer*, and their electrometers must rise and fall still farther. For by the first transference of electric fluid into B, the expelling power of A is diminished, and the superior attraction of the redundant matter in the adjacent side of B is also counteracted by the repulsion of the fluid which has entered into it; therefore no more will follow unless these forces be increased, at least to their former degree. When this addition has been made to B, and this abstraction from A, their respective electrometers must be affected. All this is in perfect conformity to experience.

75. *Second*, All the phenomena of communicated electricity must be more remarkable in proportion to the conducting power of the bodies. A very imperfect conductor, such as glass or sealing wax, will impart or receive fluid only between the very nearest parts; whereas a metalline body is instantly affected through its whole extent. This deduction is perfectly agreeable to the whole train of electric experiments. The finger receives a strong spark from a large metalline electrified body, which discharges *every part of it* of a portion of its electricity. But an excited globe, which shews, by its action on a distant body, as great a degree of

electricity, will give only a very small spark; and it is found not to be affected at any considerable distance from the point of its surface from which the transference was made. The whole electricity of a perfect conductor is discharged by touching it; but a nonconductor will successively give sparks, if touched in many different parts; and it may be seen by a nice electrometer, that each contact takes away the electricity only from a very small space round it: and it is further highly deserving of notice, that some time after a spark has been obtained from a particular spot of the electric, a second spark may be obtained from it, the electricity of the neighbouring parts having been gradually diffused through it.

76. *Third*, If an electrified conducting body touch any thing communicating with the ground by perfect conductors, all its electricity must disappear, and none can appear in the body touched by it; for the mass of the earth bears such an unmeasurable proportion to that of the greatest body that we can electrify, that when the redundancy or deficiency is divided between them, it must be imperceptible in both.

77. Hence the necessity of *insulation*, as it is called, or the surrounding by non-conductors every body which we would have exhibit electric appearances*. But we must consider, in its proper place, the manner in which the electric fluid is dissipated by imperfectly insulating substances; a subject intimately connected with the theory.

78. *Fourth*, Any unelectrified body will be first attracted by an electrified body, will touch it, and will then be repelled. The neutral body is rendered electrical by induction. It is, *in consequence of this*, attracted, comes near enough to receive a spark, or even touches it, and is then electrified by communication; and, *in consequence of this*, it is repelled.

* See the Article ELECTRICITY in the Edinburgh Encyclopædia for more particular information on this subject.

This is confirmed by an endless train of experiments. It was first taken notice of (we think) by Sir Isaac Newton. Otho Guericke, a gentleman of Magdeburgh, to whom we owe the air pump, mentions many instances of the repulsion, but did not observe that it was an universal law. Newton was so struck with it as to engage in a considerable train of experiments in the early part of his life, while meditating on the power of gravity; but even his sagacious mind did not observe the whole process of nature in his experiments. He observed, that the light bodies which rose and adhered to the rubbed plate of glass were soon after repelled by it; but did not observe, that the same piece would again rise to the glass after it had touched the table. This fact is now the foundation of many experiments, which the itinerant electricians vie with each other in rendering very amusing. We may render them instructive. Take away the middle conductor B (fig. 11.), and hang in its place a cork ball by a long silk thread. As soon as the electric body D is brought near to A, the ball is attracted by its remote end, comes into contact, is repelled by it, and attracted by the adjacent end of C, touches it, is faintly repelled by it, and again attracted by A; and the operation is repeated several times. When all has ceased, remove C, and also the electric D. C is found to have the same electricity with D, and A has the opposite electricity. The process is too obvious to need any detailed application of the theory. The cork ball was the carrier of fluid from A to C if D was electric by redundancy, or from C to A if D was undercharged. If instead of removing C when the vibrations of the ball have ceased, we bring D a little nearer, they will be renewed, and, after some time, will again cease. The reason is plain. The carrier ball had brought the conductor A into a state of equilibrium with the action of D. But this action is now increased, and the effects are renewed. If we now remove D, the ball will vibrate between A and C with great rapidity for a considerable time before the vibra-

tions come to an end; and we shall find their number to be the same as before. The cause of this is also obvious from the theory. We may suppose A to be negative, and C positive. One of them will attract the ball into contact, and will repel it, having put it into an electric state opposite to that of the other conductor. It now becomes a carrier of fluid from the positive to the negative conductor, till it nearly restore both to their primitive state of neutrality.

79. There is frequently a seeming capriciousness in those attractions and repulsions. A pith-ball, or a down feather, hung by silk, will cling to the conductor, or otherwise electrified body, and will not fly off again, at least for a long while. This only happens when those bodies are so dry as to be almost non-conductors. They acquire a positive and negative pole, like an iron nail adhering to a magnet, and are not repelled till they become almost wholly positive or negative. It never happens with conducting light bodies.

80.—*Fifth*, It should follow from the theory, that the electric attractions and repulsions will not be prevented by the intervention of non-conducting substances in their neutral state. Accordingly, it is a fact, that the interposition of a thin pane of glass, let it be ever so extensive, does not hinder the electrometer from being affected. Also, if an insulated electric be covered with a glass bell, an electrometer on the outside will be affected. Nay, a metal ball, covered to any thickness with sealing wax, when electrified, will affect an electrometer in the same way as when naked. We cannot see how these facts can be explained by the action of electric atmotspheres. It is indeed said, that the atmosphere on one side of the glass produces an atmosphere on the other; but we have no explanation of this production. If the interposed plate be a non-conductor, how does the one atmosphere produce the other? It must produce this effect by acting at a distance on the particles which are to form this atmosphere. Of what use, then, is the atmosphere, even if those atmospheres could effect the observed

motions of the electrometer in consistency with the laws of mechanics! The atmospheres only substitute millions of attractions or repulsions in place of one. We must observe, however, that the motions of the electrometer are modified, and sometimes greatly changed, by the interposed non-conducting plate; but this is owing to the electricity induced on the plate. If the electric is positive, the adjacent surface of the plate becomes faintly negative, and the side next the electrometer slightly positive. This affects the electrometer even more than the more remote electric does. That this is the cause of the difference between the state of the electrometer when the plate is there and when it is removed, will appear plainly by breathing gently on the glass plate to damp it, and give it a small conducting power. This will make some change in the position of the electrometer. Continue this more and more, till the plate will no longer insulate. The changes produced on the electrometer's position will form a regular series, till it is seen to assume the very position which it would have taken had the plate been brass. Then, considering those changes in a contrary order, and supposing the series continued a little farther, we shall always find that it leads to the position which it would have taken when no plate whatever is interposed. We consider this as an important fact, shewing that the electric action is similar to gravitation, and that there is no more occasion for the intervention of an atmosphere for explaining the phenomena of electricity than for explaining those of gravitation.

81.—*Sixth*, Since non-electrics are conductors, and since electrics may be excited by friction with a non-electric, it follows, that if this non-electric be insulated, and separated from the electric, it will exhibit signs of electricity; but when they are together, there must not appear any marks of it, however strong the excitation may be. We do not pretend to comphrehend distinctly the manner in which friction, or the other modes of excitation, operate in changing the con-

nection between the particles of the fluid and those of the tangible matter; nor is this explained in any electric theory that we know: but if we are satisfied with the evidences which we have for the existence of a substance, whose presence or absence is the cause of the electric phenomena, we must grant that its usual connection with the tangible matter of bodies is changed in the act of excitation, by friction, or by any other means. In the case of friction producing positive electricity on the surface of the electric, we must suppose that the act of friction causes one body to emit or absorb the fluid more copiously than the other, or perhaps the one to emit, and the other to absorb. Which ever is the case, the adjoining surfaces must be in opposite states, and the one must be as much overcharged as the other is undercharged. When the bodies (which we may suppose to have the form of plates) are joined, and the one exactly covers the other, the assemblage must be inactive; for a particle of moveable fluid, situated any where on the side of the overcharged plate, will be as much attracted by the undercharged surface of the remote plate as it is repelled by the overcharged surface of the near plate. The surfaces are equal, and equally electric, and act on either side with equal intensity; and they are coincident. Therefore their actions balance. The action is expressed by the formula of § 43; namely, $F' m' \times \overline{z - z'}$; and $z - z'$ is $= 0$, by reason of the equal distances of these surfaces from the particle of exterior fluid.

But let the plates be separated. Part, and probably the greatest part, of the redundant fluid on one of the rubbed surfaces will fly back to the other, being urged both by the attraction of the redundant matter and the repulsion of its own particles. But the electric, being electric because, and only because, it is a non-conductor, must retain some, or will remain deprived of some, in a stratum a little within the surface. The two plates must therefore be left in op-

posite states, and the conducting, or non-electric plate, if insulated before separation, must now exhibit electric action.

All this is exactly agreeable to fact. We also know that electrics may be excited by rubbing on each other; and if of equal extent, and equally rubbed, they exhibit no electric powers while joined together; but when parted, they are always in opposite states. The same thing happens when sulphur is melted in a metal dish, or when Newton's metal is melted in a glass dish. While joined, they are most perfectly neutral; but manifest very strong opposite electricities when they are separated. This completely disappears when they are joined again, and re-appears on their separation, even after being kept for months or years in favourable circumstances. We have observed the plates of talc, and other laminated fossils, exhibit very vivid electricity when split asunder.

82. Attention to these particulars enables us to construct machines for quickly exciting vivid electricity on the surface of bodies, and for afterwards exhibiting it with continued dispatch. The whirling globe, cylinder, or plate, first employed by Mr. Hauksbee, for the solitary purpose of examining the electricity of the globe, was most ingeniously converted by Hausen, a German professor, into a rapid collector and dispenser of electricity to other bodies, by placing an insulated prime conductor close to that part of the surface of the globe which had been excited by friction. Did our limits give us room, we should gladly enlarge on this subject, which is full of most curious particulars, highly meriting the attention of the philosopher. But it might easily occupy a whole volume; and we have still before us the most interesting parts of the mechanical department of electricity, and shall hardly find room for what is essentially requisite for a clear and useful comprehension of it. We must, therefore, request our readers to have recourse to the original authors, who have considered the excitation by friction minutely. And we particularly recommend the

very careful perusal of Beccaria's Dissertations on it, comparing the phenomena, in every step, with this theory of Æpinus. Much valuable information is also obtained from Mr. Nicholson's observations*. The Æpinian theory will be found to connect many things, which, to an ordinary reader, must appear solitary and accidental.

83. Seeing that this very simple hypothesis of Æpinus so perfectly coincides in its legitimate consequences with all the general phenomena of attraction and repulsion, and not only with those that are simple, but even such as are compounded of many others—we may listen, without the imputation of levity, to the other evidences which may be offered for the materiality and mobility of the cause of those mechanical phenomena. Such evidences are very numerous, and very persuasive. We have said, that the transference of electricity is desultory, and that the change made in the electric state of the communicating bodies is always considerable. It appears to keep some settled ratio to the whole electric power of the body. When the form of the parts where the communication takes place, and other circumstances, remain the same, the transference increases with the size of the bodies; and all the phenomena are more vivid in proportion. When the conductor is very large, the spark is very bright, and the snap very loud.

1. This snap alone indicates some material agent. It is occasioned by a sonorous undulation of the air, or of some elastic fluid, which suddenly expands, and as suddenly collapses again. But such is the rapidity of the undulation, that when it is made in close vessels it does not exist long enough, in a very expanded state, to affect the column of water, supported in a tube by the elasticity of the air, for the purpose of a delicate thermometer or barometer; just

* A full account of Nicholson's Experiments will be found in the *Phil. Trans.* 1789 p. 265. and in the EDINBURGH ENCYCLOPÆDIA, Art. ELECTRICITY, vol. viii. p. 510

as a musket ball will pass through a loose hanging sheet of paper without causing any sensible agitation.

2. The spark is accompanied by intense heat, which will kindle inflammable bodies, will melt, explode, and calcine metals.

3. The spark produces some very remarkable chemical effects. It calcines metals even under water or oil; it renders Bolognan phosphorus luminous: It decomposes water, and makes new compositions and decompositions of many gaziform fluids; it affects vegetable colours; it blackens the calces of bismuth, lead, tin, luna cornea; it communicates a very peculiar smell to the air of a room, which is distinct from all others; and in the calcination of metals, it changes remarkably the smells with which this operation is usually accompanied: it affects the tongue with an acidulous taste; it agitates the nervous system.—When we compare these appearances with similar chemical and physiological phenomena, which naturalists never hesitate in ascribing to the action of material substances, transferable from one body, or one state of combination, to another, we can see no greater reason for hesitating in ascribing the electric phenomena to the action of a material substance; which we may call *a fluid*, on account of its connected mobility, and *the electric fluid*, on account of its distinguishing effects. We are well aware, that these evidences do not amount to demonstration; and that it is possible that the electric phenomena, as well as many chemical changes, may result from the mere difference of arrangement, or position, of the ultimate particles of bodies, and may be considered as the result of a change of modes, and not of things. But in the instances we have mentioned, this is extremely improbable.

We therefore venture to assume the existence of this substance, which philosophers have called *the electric fluid*, as a proposition abundantly demonstrated; and to affirm, on the authority of all the above-mentioned facts, that its me-

chanical character is such as is expressed in Mr. Æpinus's hypothesis.

We proceed, therefore, to explain the most interesting phenomena of electricity from these principles.

84. We have seen that, in a perfect conductor, in its natural state, the electric fluid is uniformly distributed, and cannot remain in any other condition. We are particularly interested to know how it is distributed in an overcharged or undercharged body, and how this is affected by the circumambient non-conducting air. It is evident that much depends on this. The tendency to escape, and, particularly, the tendency to transference from one body to another, must be greatest where the fluid is most constipated. We know that it tends remarkably to dissipate from all protuberances, edges, and long bodies, and that it is impossible to confine it in a body having very acute far-projecting points; and, what is more paradoxical, it is hardly possible to prevent its entering into a body furnished with a sharp point. The smallest reflection must suggest to our imagination, that a perfectly moveable fluid, whose particles mutually repel, even at considerable distances, and which is confined in a vessel from which it cannot escape, must be compressed against the sides of the vessel, and be denser there than in the middle of the vessel. But in what proportion its density will diminish as we recede from the walls of the vessel, must depend on the change of electric repulsion by an increase of distance. The intensity varies in the proportion of some function of the distance, and may be expressed by the ordinates of a curve, on whose axis the distances are measured. But we are ignorant of this function. We must therefore endeavour to discover it, by observing a proper selection of phenomena. Having made some approximation to this discovery, such as shall give rise to a *probable conjecture* concerning the function which expresses the intensity of electric repulsion, mathematics will then enable

us to say how the fluid must be distributed (at least in some simple and instructive cases) in a perfectly conducting body surrounded by the air, and what will be its action on another body. Thus we shall obtain ostensible results, which we can compare with experiments. The writer of this article made many experiments with this view above 30 years ago, and flatters himself that he has not been unsuccessful in his attempts. These were conducted in the most obvious and simple manner, suggested by the reasonings of Mr. Æpinus; and it was with singular pleasure that, some years after, he perused the excellent dissertation of Mr. Cavendish in the Philosophical Transactions, vol. 61. where he obtained a much fuller conviction of the truth of the conclusion which he had drawn, in a ruder way, from more familiar appearances. Mr. Cavendish, has, with singular sagacity and address, employed his mathematical knowledge in a way that opened the road to a much farther and more scientific prosecution of the discovery, if it can be called by that name. After this, Mr. Coulomb, a distinguished member of the French academy of sciences, engaged in the same research in a way still more refined; and supported his conclusions by some of the most valuable experiments that have been offered to the public. We shall now give a very brief account of this argument: and have premised these historical remarks; because the writer, although he had established the general conclusion, and had read an account of his investigation in a public society in 1769, in which it was applied to the most remarkable facts then known in electricity, has no claim to the more elaborate proofs of the same doctrine, which are given in some of the following paragraphs. These are but an application of Mr. Cavendish's more cautious and general mathematical procedure, to the function which the writer apprehends to be sufficiently established by observation.

The most unexceptionable experiments with which we can begin, seem to be the repulsions observable between

two *small* spheres. Whatever be the law of distribution of the particles in a sphere, the general action of its particles on the particles of another sphere will follow a law which will not differ much from the law of action between two particles, if the diameters of the spheres be small in proportion to their distance from each other. The investigation was therefore begun with them. But the subject required an electrometer susceptible of comparison with others, and that could exhibit absolute measures. The one employed was made in the following manner; and we give it to the public as a valuable philosophical instrument.

85. Fig. 15. represents the electrometer in front. A is a polished brass ball, $\frac{1}{4}$th of an inch in diameter. It is fixed on the point of a needle three inches long, as slender as can be had of that length. The other end of the needle passes through a ball of amber or glass, or other firm non-conducting substance, about half or three-fourths of an inch in diameter; but the end must not reach quite to the surface, although the ball is completely perforated. From this ball rises a slender glass rod FEL, three inches long from F to E, where it bends at right angles, and is continued on to L, immediately over the centre of the ball A. At L is fixed a piece of amber C, formed into two parallel cheeks, between which hangs the stalk DCB of the electrometer. This is formed by dipping a strong and dry silk thread, or fine cord, in melted sealing wax, and holding it perpendicular till it remain covered with a thin coating, and be fully penetrated by it. It must be kept extended, that it may be very straight; and it must be rendered smooth, by holding it before a clear fire. This stalk is fastened into a small cube of amber, perforated on purpose, and having fine holes drilled in two of its opposite sides. The cheeks of the piece C are wide enough to allow this cube to move freely between them, round two fine pins, which are thrust through the holes in the cheeks, and reach about half way to the stalk. The lower part of the stalk is about three inches

long, and terminates in a gilt and burnished cork-ball (or a ball of thin metal), a quarter of an inch in diameter. The upper part CD is of the same length, and passes, with some friction, through a small cork-ball. This part of the instrument is so proportioned, that when FE is perpendicular to the horizon, and DCB hangs freely, the balls B and A just touch each other. Fig. 16. gives a side perspective view of the instrument. The ball F is fixed on the end of the glass rod FI, which passes perpendicularly through the centre of a graduated circle GHO, and has a knob handle of boxwood on the farther end I. This glass rod turns stiffly, but smoothly, in the head of the pillar HK, &c. and has an index NH, which turns round it. This index is set parallel to the line LA, drawn through the centre of the fixed ball of the electrometer. The circle is divided into 360 degrees, and 0 is placed uppermost, and 90 on the right hand. Thus the index will point out the angle which LA makes with the vertical. It will be convenient to have another index, turning stiffly on the same axis, and extending a good way beyond the circle.

This instrument is used in the following manner: A connection is made with the body whose electricity is to be examined, by sticking the point of the connecting wire into the hole at F till it touch the end of the needle: or if we would merely electrify the balls A and B, and then leave them insulated, we have only to touch one of them with an electrified body. Now take hold of the handle I, and turn it to the right till the index reach 90. In this position, the line LA is horizontal, and so is CB: and the moveable ball B is resting on A, and is carried by it. Now electrify the balls, and gently turn the handle backwards, bringing the index back towards 0, &c. noticing carefully the two balls. It will happen that, in some particular position of the index, they will be observed to separate. Bring them together again, and again cause them to separate, till the exact position at separation is ascertained. This will shew their

repulsive force in contact, or at the distance of their centres, equal to the sum of their radii. Having determined this point, turn the instrument still more toward the vertical position. The balls will now separate more and more. Let an assistant turn the long index so as to make it parallel to the stalk of the electrometer, by making the one hide the other from his view. The mathematical reader will see that this electrometer has the properties ascribed to it. It will give absolute measures: for by poizing the stalk, by laying some grains weight on the cork-ball D, till it becomes horizontal and perfectly balanced, and computing for the proportional lengths of BC and DC, we know exactly the number of grains with which the balls must repel each other (when the stalk is in a horizontal position), in order *merely to separate*. Then a very simple computation will tell us the grains of repulsion when they separate in any oblique position of the stalk; and another computation, by the resolution of forces, will shew us the repulsion exerted between them when AL is oblique, and BC makes any given angle with it. All this is too obvious to need any farther explanation. The reason for giving the connection between A and C such a circuitous form, was to avoid all action between the fixed and the moveable part of the electrometer, except what is exerted between the two balls A and B. The needle AF, indeed, may act a little, and might have been avoided, by making the horizontal axis FI to join with A: but as it was wanted to make the instrument of more general use, and frequently to connect it with an electrical machine, a battery, or a large body, no mode of connection offered itself which would not have been more faulty in this respect. The neatest and most compendious form would have been to attach the axis FI to C, and to make CA and CB stiff metalline wires, in the same manner as Mr. Brookes's electrometer is made. But as the whole of their lengths would have acted, this construction would have been very improper in the investigation of the law of

electric repulsion. As it now stands, we imagine that it has considerable advantages over Mr. Brookes's construction; and also over Mr. De Luc's comparable electrometer, described in his Essays on Meteorology. It has even advantages over Mr. Coulomb's incomparably more delicate electrometer, which is sensible, and *can measure* repulsions which do not exceed the 50,000 of a grain; for the instrument which we have described will measure the *attractions* of the oppositely electrified bodies; a thing which Mr. Coulomb could not do without a great circuit of experiments. For instead of making the ball B *above* A, by inclining the instrument to the right hand, we may incline it to the left; and then, by electrifying one of the balls positively, and the other negatively, when at a great distance from each other, their mutual attraction will cause them to approach; CB will deviate from the vertical toward A; and we can compute the force by means of this deviation.

We must remind the person who would make observations with this instrument, that every part of it must be secured against dissipation as much as possible, by varnishing all its parts, by having all angles, points, and roughnesses removed, and by choosing a dry state of the air, and a warm room; and, because it is impossible to prevent dissipation altogether, we must make a previous course of experiments, in a variety of circumstances, in order to determine the diminution per minute corresponding to the circumstances of the experiments that are to be made with further views.

We trust that the reader will accept of this particular account of an instrument which promises to be of considerable service to the curious naturalist; and we now proceed with an account of the conclusions which have been drawn from observations made with it.

Here we could give a particular narration of some of the experiments, and the computations made from them; but we omit this, because it is really unnecessary. It suffices to say, that the writer has made many hundreds, with differ-

ent instruments, of different sizes, some of them with balls of an inch diameter, and radii of 18 inches. Their coincidence with each other was far beyond his expectation, and he has not one in his notes which deviate from the medium $\frac{1}{8}$ of the whole force, and but few that have deviated $\frac{1}{12}$. The deviations were as frequently in excess as in defect. His custom was to measure all the forces by a linear scale, and express them by straight lines erected as ordinates to a base, on which he set off the distances from a fixed point; he then drew the most regular curve that he could through the summits of these ordinates. This method shews, in the most palpable manner, the coincidence or irregularity of the experiments.

86. The result of the whole was, that the mutual repulsion of two spheres, electrified positively or negatively, was very nearly in the inverse proportion of the squares of the distances of their centres, or rather in a proportion somewhat greater, approaching to $\frac{1}{x^{2.06}}$. No difference was observed although one of the spheres was much larger than the other; and this circumstance enables us to make a considerable improvement on the electrometer. Let the ball A be made an inch in diameter, while B is but $\frac{1}{4}$ of an inch. This greatly diminishes the proportion of the irregular actions of the rest of the apparatus to the whole force, and also diminishes the dissipation when the general intensity is the same.

87. When the experiments were repeated with balls having opposite electricities, and which therefore attracted each other, the results were not altogether so regular, and a few irregularities amounted to $\frac{1}{6}$ of the whole; but these anomalies were as often on one side of the medium as on the other. This series of experiments gave a result which deviated as little as the former (or rather less) from the inverse duplicate ratio of the distances; but the deviation was in defect as the other was in excess.

We therefore think that it may be concluded, that the action between two spheres is exactly in the inverse duplicate ratio of the distance of their centres, and that this difference between the observed attractions and repulsions is owing to some unperceived cause in the form of the experiment.

88. It must be observed also, that the attractions and repulsions, with the same density and the same distances, were, to all sense, equal, except in the forementioned anomalous experiments. The mathematical reader will see, that the above-mentioned irregularities are imperfections of experiment, and that the gradations of this function of the distances are too great to be much affected by such small anomalies. The indication of the law is precise enough to make it worth while to adopt it, in the mean time, *as a hypothesis*, and then to select, with judgment, some legitimate consequences which will admit of an exact comparison with experiment, on so large a scale, that the unavoidable errors of observation shall bear but an insignificant proportion to the whole quantity. We shall attempt this: and it is peculiarly fortunate, that this observed law of action between two spheres gives the most easy access to the law of action between the particles which compose them; for Sir Isaac Newton has demonstrated (and it is one of his most precious theorems,) that if the particles of matter act on each other with a force which varies in the inverse duplicate ratio of the distances, then spheres, consisting of such particles, and of equal density at equal distances from the centre, also act on each other with forces varying in the same proportion of the distances of their centres. He demonstrates the same thing of hollow spherical shells. He demonstrates that they act on each other with the same force as if all their matter were collected in their centres. And, lastly, he demonstrates that if the law of action between the particles be different from this, the sensible action of spheres

or of hollow spherical shells, will also be different (see *Principia*, I. Prop. 74.)

89. Therefore we may conclude, that the law of electric attraction and repulsion is similar to that of gravitation, and that each of those forces diminishes in the same proportion that the square of the distance between the particles increases. We have obtained much useful information from this discovery. We have now full confirmation of the propositions concerning the mutual action of two bodies, each overcharged at one end and undercharged at the other. Their evidence before given amounted only to a reasonable probability; but we now see, that the curve line, whose ordinates represent the forces, is really convex to the abscissa, and that $Z + z'$ is always greater than $Z' + z$; from which circumstance all the rest follows of course.

90. Let us now inquire into the manner in which the redundant fluid, or redundant matter, is distributed in bodies; the proportion in which it subsists in bodies communicating with each other; the tendencies to escape; the forces which produce a transference, &c. &c.

In the course of this inquiry, a continual reference will be made to the following elementary proposition:

91. Let ABD (fig. 17.) be the base of a cone or pyramid, whose vertex is P, and axis PC; and let *a b d* be another section of it by a plane parallel to the base; let these two circles, or similar polygons, consist of matter or fluid of equal and uniform density; and let P be a particle of fluid or matter; the attraction or repulsion of this particle for the whole matter or fluid in the figure ABD is equal to its attraction or repulsion for the whole matter or fluid in *a b d*. For the attraction for a particle in ABD is to the attraction for a particle similarly placed in *a b d* as Pc^2 to PC^2; and the number of particles in ABD is to that of those in *a b d* as PC^2 to Pc^2; therefore the whole attraction for ABD is to that for *a b d* as $Pc^2 \times PC^2$ to $PC^2 \times Pc^2$, or in the ratio of equality.

Cor. 1. The same will be true of the action of plates of equal thickness and equal density; or, in general, having such thickness and density as to contain quantities of matter or fluid proportional to their areas.

2. The action of all such sections made by parallel planes, or by planes equally inclined to their axis, are equal.

3. The tendency of a particle P to a plane, or plate of uniform thickness and density, and infinitely extended, or to a portion of it bounded by the same pyramid, is the same, at whatever distance it be placed from the plate, and it is always perpendicular to it.

4. This tendency is proportional to the density and thickness of the plate or plates jointly.

It is only in two or three simple cases that we can propose to state with precision what will be the disposition and action of the electric fluid in bodies; but we shall select those that are most instructive, and connected with the most remarkable and important phenomena.

92. Let A *a d* D (fig. 18.) and E *e h* H represent the sections of a part of two infinitely extended parallel plates (which we shall call A and E), consisting of solid conducting matter, in which the electric fluid can move without any obstruction, but from which it cannot escape.

First, Let them be both overcharged, A containing the quantity r of redundant fluid, and E containing the quantity s, and let r be greater than s.

The fluid will be disposed in the following manner:

1. There will be two strata, A *a b* B and G *g h* H, adjoining to the remote surfaces, in each of which the quantity $\frac{r+s}{2}$ will be crowded together as close as possible.

2. Adjoining to the interior surface (that is, the surface nearest to E) of the plate A, there will be a stratum C *c d* D_s containing the quantity $\frac{r-s}{2}$ crowded together.

3. The adjacent side of E will have a stratum E *e f* F, just sufficient for containing the quantity $\frac{r-s}{2}$ at its natural density. This stratum will be entirely exhausted of fluid.

4. The spaces B *b c* C and F *f g* G will be in their natural state.

For a particle of fluid in the space B *b c* C is urged in the direction *a d* by the force $\frac{r+s}{2}$ (§ 91, 3.) and in the direction *d a* by the force $\frac{r-s}{2}$, therefore it is, on the whole, urged in the direction *a d* with the force *s*, which will balance the repulsion of the redundant fluid in the other plate. A particle of fluid in the space F *f g* G is repelled in the direction *h e* by a force $\frac{r+s}{2}$ by the fluid in G *g h* H, and it is attracted in the same direction by the redundant matter in E *e f* F, with the force $\frac{r-s}{2}$. These make a force *r* which balances the repulsion *r* of the other plate. No other disposition will be permanent; for if a particle be taken out from either stratum A *a b* B or C *c d* D into the space between them, the repulsion from that stratum which it quitted is lessened, and the repulsion of the opposite stratum, joined to that of the other plate, will drive it back again. The same thing holds with respect to the fluid in the other plate.

93. *Cor.* 1. If the two plates be equally overcharged, all the redundant fluid will be crowded on the remote surfaces, and the adjacent surfaces will be in the natural state.

94. In the *second* place, let the plates be undercharged, and let *r* be the fluid wanting in A, and *s* the fluid wanting in E, and let *s* be greater than *r*; then,

1. The Strata adjoining to A *a* and H *h* will be completely exhausted of fluid, and the redundant matter in each will be such as would be saturated by $\frac{r+s}{2}$.

2. The stratum C *c d* D will contain redundant fluid $\frac{s-r}{2}$, crowded close.

3. The stratum E *e f* F will be deprived of fluid, and the quantity abstracted is $\frac{s-r}{2}$.

4. The spaces B *b c* C and F *f g* G are in the natural state.

The demonstration is the same as in the former case.

95. *Thirdly*, Let A be overcharged, and E undercharged, A containing the redundant fluid r, and E wanting the fluid s; and let r be greater than s. Then,

1. The strata A *a b* B and G *g h* H contain the redundant fluid $\frac{r-s}{2}$, crowded close.

2. The stratum C *c d* D contains the quantity $\frac{r+s}{2}$, crowded close.

3. The stratum E *e f* F is exhausted, and wants the quantity $\frac{r+s}{2}$.

4. The rest is in the natural state.

96. *Cor.* 2. If the redundant fluid in A be just sufficient to saturate the redundant matter in E, the two remote surfaces will be in their natural state, all the redundant fluid in A being crowded into the stratum C *c d* D, and all the redundant matter being in E *e f* F.

This disposition will be the same, whatever is the distance or thickness of the plates, unless the redundant fluid in A be more than can be contained in the whole of E when crowded close.

97. When the two plates are overcharged, the fluid presses their remote surfaces with the force $\frac{\overline{r+s}^2}{4}$, and would escape with that force if a passage were opened. It would enter the remote surfaces of two undercharged plates with the

same force; and, in either case, it would run from the inner surface of one to the adjacent surface of the other, with the force $\frac{\overline{r-s}^2}{4}$.

If one be overcharged and the other undercharged, fluid would escape from the remote surface with the force $\frac{\overline{r-s}^2}{4}$, and would run through a canal between them with the force $\frac{\overline{r+s}^2}{4}$.

They repel or attract each other with the force $\overline{r+s}^2$, according as they are both over or undercharged, or as one is overcharged and the other undercharged.

This example of parallel plates, infinitely extended, is the simplest that can be supposed. But it cannot obtain under our observation; and in all cases which we can observe, the fluid cannot be uniformly spread in any stratum, but must be denser near the edges, or near the centre, as they are overcharged or undercharged.

98. Let ABD (fig. 19.) represent a sphere of perfectly conducting matter, overcharged with electric fluid, which is perfectly moveable in its pores, but cannot escape from the sphere. Let it be surrounded by conducting matter saturated with moveable fluid. It is required to determine the disposition of the fluid within and without this sphere.

Sir Isaac Newton has demonstrated (*Princ.* I. 70.) that a particle p, placed anywhere within this sphere, is not affected by any matter that is without the concentric spherical surface $p\ q\ r$ in which itself is situated, therefore not affected by what is between the surfaces ABD and $p\ q\ r$. He also demonstrates, that the matter within the surface $p\ q\ r$ acts on the particle p in the same manner as if the whole of it were collected in the centre C.

Hence it follows, that the redundant fluid will be all constipated as close as possible within the external surface of the

sphere, forming a shell of a certain minute thickness, between the spherical surfaces ABD and *a b d*: and all that is within this (that is, nearer the centre C,) will be in its natural state.

With respect to the distribution of the fluid in the surrounding matter, which we suppose to be infinitely extended, we must recollect that this shell of constipated redundant fluid repels any external particle of fluid in the same manner as if all were collected at C. Hence it is evident, that the fluid in the surrounding matter will be repelled, and, being moveable, it will recede from this centre: and there will be a space all round the sphere ABD which is undercharged, forming a shell between the concentric surfaces ABD and *a ε ϛ*. This shell will contain such a quantity of redundant matter, that its attraction for a particle of fluid is equal to the repulsion of the shell of fluid crowded internally on the surface ABD. All beyond this surface *a ε ϛ* will be in its natural state: for this redundant matter acts on a particle of fluid, situated farther from the centre, in the same manner as if all this redundant matter were collected in the centre C. So does the redundant fluid in the constipated shell. Therefore their actions balance each other, and there is no force exerted on any particle of fluid beyond this deficient shell. This deficient shell will not affect the fluid in the sphere *a b d* by Newton's demonstration. No other disposition will be permanent. But farther: This undercharged shell must be completely exhausted: for a particle of fluid placed between ABD and *a ε ϛ* will be more repelled by the fluid in the crowded shell within the surface ABD, than it is attracted by the redundant matter of its own shell that is less remote from the centre: and it is not affected by what is more remote from the centre. Therefore the fluid without the sphere ABD cannot be in equilibrio, unless the shell between ABD and *a ε ϛ* be not only rarefied, but altogether exhausted of fluid.

If the sphere be undercharged, the space between ABD and *a b d* will be entirely exhausted of fluid, and there will be a shell $\alpha\beta\delta$ of redundant matter surrounding the sphere. All within *a b d*, and all without $\alpha\beta\delta$, will be in its natural state. It is unnecessary to repeat the steps of the same demonstration.

This valuable proposition is by the Hon. Mr. Cavendish.

99. This would be the disposition in and about a glass globe filled and surrounded with an ocean of water, and having redundant fluid within it, on the supposition that glass is impervious to the electric fluid. But it would not affect an electrometer, even supposing that the movements of the electrometer could be effected under water. Suppose the globe of water to be surrounded with air, and that the fluid is disposed in both in the manner here described; it will be perfectly neutral in its action on any electrometer situated in the air. But, by reason of the almost total immobility of the fluid in pure dry air, this state cannot soon obtain; and, till it obtain, the constipated shell within the glass must repel the fluid in an electrometer more than the partially rarefied shell of air, which surrounds the glass, attracts it. By the gradual retiring of the fluid in the surrounding air from the globe, the attraction of the deserted matter will come nearer to equality with the repulsion of the constipated shell within the glass, and the globe will appear to have lost fluid. Yet it may retain all the redundant fluid which it had at the first. Therefore we are not to imagine that a body similar to this globe has no redundant electric fluid, or only a small quantity, because we observe it inactive, or nearly so.

100. Thus we see, as we proceed, that the Æpinian theory is adequate to the explanation of the phenomena. But we see it much more remarkably in a very familiar and amusing experiment, usually called the ELECTRIC WELL. See ELECTRICITY, *Encycl.* Sect. x. 4.

To see it in perfection, make a glass vessel of globular shape, with a narrow mouth, sufficiently wide, however, to admit an electrometer suspended to the end of a glass rod of a crooked form, so that the electrometer can be presented to any part of the inside. Smear the outside of the globe with some transparent clammy fluid, such as syrup. Set it on an insulating stand (a wine glass), and electrify it positively. Hold the electrometer near it, any where without, and it will be strongly affected. Its deviations from the perpendiculár (if the ball of the electrometer has also been electrified) will indicate a force inversely as the square of the distance from the centre of the globe, pretty exactly, if the thread of the electrometer is of silk. Now let down the electrometer into the inside of the globe. It will not be affected in any sensible degree, nor approach or avoid any body that is lying within the globe. The electrometer may be held in all parts of the globe, and when brought out again, is perfectly inactive and neutral. But if the balls of the electrometer be touched with a wire, while hanging free within the globe, they will, on withdrawing the wire, repel each other; and when taken out, they will be found negatively electrified. The experiment succeeds as well with a metal globe; nay, even although the mouth be pretty wide; in which case, there is not a perfect balance of action in every direction. The electrometer may be made to touch the bottom of the globe, or any where not too near the mouth, without acquiring any sensible electricity; but if we touch the outside with the electrometer, it will instantly be electrified and strongly repelled. Deep cylinders, and all round vessels with narrow mouths, exhibit the same faintness of electricity within, except near the brims, although strongly electric without; and even open metal cups have the interior electricity much diminished.

101. Reflecting on this valuable proposition of Mr. Cavendish, we see clearly why an overcharged electric is only superficially so; and that this will be the case even although

we attempt to accumulate a great quantity of electricity in it, by melting it in a thin glass globe, and electrifying it while liquid, and keeping up the accumulating force till it becomes quite cold. The present writer, not having considered the subject with that judicious accuracy that Mr. Cavendish exerted, had hopes of producing a powerful and permanent electric in this way, and was mortified and puzzled by the disappointment, till he saw his mistake on reading Mr. Cavendish's dissertation.

These observations also point out a thing which should be attended to in our experiments for discovering the electricity excited in the spontaneous operations of nature, as in chemical composition and decomposition, congelation, fusion, evaporation, &c. It has been usual to put the substances into glass, or other non-conducting vessels, or into vessels which conduct very imperfectly. In this last case especially, the very faint electricity which is produced, instantly forms a compensation to itself in the substance of the vessel, and the apparatus becomes almost neutral, although there may have been a great deal of electricity excited. It will be proper to consider, whether the nature of the experiment will admit of metalline vessels. In the experiments on metalline solutions, the best method seems to be, to make the vessel itself the substance that is to be dissolved.

102. For similar reasons we may collect, without a more minute examination, that bodies of all shapes, when overcharged, will have the redundant fluid much denser near the surface than in the interior parts; and denser in all elevations, bumps, projections, angles, and near the ends of oblong bodies; and that, in general, the quantity of redundant fluid or redundant matter, will be much more nearly proportional to the surfaces of bodies than to their quantities of matter. All this is fully proved by experience. The experiment of the electrified chain is a very beautiful one. Lay a long metal chain in an insulated metal dish furnished with an electrometer. Let one end be held an inch or two above

the coil by a silk thread. Electrify the whole, and observe the divergency of the electrometer; then, gradually drawing up the chain from the coil, the electrometer will gradually fall lower, and lowering the chain again will gradually raise it.

103. We now see with how little reason Lord Mahon concluded that the point of his conductor, observed to be neutral, corresponded with his theory; namely, one of the media of a harmonic division. We see no reason for beginning the computation at the extremity of the prime conductor. It certainly should not have been from the extremity. Had the prime conductor been a single globe, it should have begun from the centre of this globe. If it was of the usual form, with an outstanding wire, terminated by a large ball, the action of the body of the conductor should certainly have been taken into the account. In short, almost any point of the long conductor might have been accommodated to his Lordship's theory.

104. We might now proceed to investigate the distribution of the electric fluid in bodies exposed to the action of others, and particularly in the oblong conductors made use of in our preparatory propositions. The problem is determinate, when the length and diameter of cylindric conductors are given; but even when the electric employed for inducing the electricity is in the form of a globe, we must employ functions of the distances that are pretty complex, and oblige us to have recourse to second fluxions. The mutual actions of two oblong conductors, of considerable diameters, give a problem that will occupy the first mathematicians; but which is quite improper for this scanty abstract. Nor is a minute knowledge of the disposition of the fluid of very important service. We may therefore content ourselves with a general representation of the state of the fluid in the following manner, which will give us a pretty distinct notion how it will act in most cases:

Let A (fig. 20.) be an overcharged sphere, and BC a conducting cylindric or prismatic body; draw *b c* parallel to BC, and erect perpendiculars B *b*, C *c*, P *p*, &c. to represent the equable density of the fluid, when the conductor is in its natural state; but let B *d*, C *r*, P *t*, &c. represent the unequal densities in its different points, while in the vicinity of the overcharged sphere. These ordinates must be bounded by a line *d n r*, which will cut the line *b c* in the point *n* of the perpendicular, drawn from the neutral point N of the conductor. The whole quantity of fluid in the conductor is represented by the parallelogram BC *c b*; which must therefore be equal to the space BC *r n d*: the redundant fluid in any portion CP or PN is represented by the spaces *c r t p*, or *t p n*; and the redundant matter, or deficient fluid, in any portion BQ, is represented by *b d v q*. The action of this body on any body placed near it, depends entirely on the area contained between this curve line and its axis *b c*. The only circumstance that we can ascertain with respect to this curve is, that *the variations of curvature* in every point are proportional to the forces exerted by the sphere A; and are therefore inversely as the squares of the distances from A. This property will be demonstrated by and bye. The place of *n*, and the magnitude of the ordinates, will vary as the diameter of the conductor varies. We shall consider this a little more particularly in some cases which will occur afterwards. We may consider the simplest case that can occur; namely, when the conductor is, like a wire, of no sensible diameter, nay, as containing only one row of particles.

Let AE (fig. 21.) be such a slender conducting canal; and let B *b*, C *c*, E *e*, &c. represent the density of the fluid which occupies it, being kept in this state of inequable density by the repulsion for some overcharged body. A particle in C is impelled in the direction CE by all the fluid on the side of A, and in the direction CA by all the fluid on the side of E. The moving force, therefore, arises from

the difference of these repulsions. When the diameter of the canal is constant, this arises only from the difference of density. The force of the element adjacent to E may therefore be expressed by the excess of D d above C c, and the action at the distance CD jointly. Therefore, drawing $\beta c \varepsilon$ parallel to AE, this force of the element E will be expressed by $\frac{d\delta}{c\delta^2} \dot{x}$, repelling the particle in the direction CA. If CF be taken equal to CD, the force of the element at F will be expressed by $\frac{f\varphi}{c\varphi^2} \dot{x}$, or $\frac{f\varphi}{c\delta^2} \dot{x}$, also impelling the particle in the direction CA. The joint action of these two elements therefore is $\frac{d\delta + f\varphi}{c\delta^2} \dot{x}$. If $b\,c\,e$ were a straight line we should have $d\delta + f\varphi$ always proportional to $c\delta$; and it might be expressed by $m \times c\delta$; m being a number expressing what part of $c\delta$ the sum of $d\delta$ and $f\varphi$ amounts to (perhaps $\frac{1}{10}$th, or $\frac{1}{20}$th, or $\frac{1}{30}$th, &c.). But in the case expressed in the figure, $d\delta$ does not increase so fast as $c\delta$, and $f\varphi$ increases faster than $c\delta$. However, in the immediate neighbourhood of any point C, we may express the accelerating force tending towards A by $\frac{mc\delta}{c\delta^2} \dot{x}$ without any sensible error; that is, by $m\frac{\dot{x}}{x}$ that is, by the fluxion of the area of a hyperbola HD′G, having CC′ and CK for its asymptotes; and the whole action of the fluid between F and D, on the particle C, will be expressed by the area C′CDD′H. Hence it follows, that the action of the smallest conceivable portion of the canal immediately adjoining to C on both sides, or the difference of the actions of the two adjoining elements, is equal to the action of all beyond it. This shews, that the state of compression is hardly affected by any thing that is at a sensible distance from C; and that the density of the fluid, in an indefinitely slender canal, is, to all sense, uniform. The geometer will also see, that the second fluxion of D d is proportional to the force

of the distant body. We learn, therefore, so much of the nature of the curve *b c e.—(Coulomb).*

We are now in a condition to examine the communication of electricity by means of conducting canals (which is one of the most important articles of the study), having found that the fluid, in a very slender canal, is very nearly of uniform density throughout.

106. There can be no doubt but that, if a body B (fig. 22.) be overcharged or undercharged, any other body C, which communicates with it by a conducting canal, will also be overcharged or undercharged. It is as evident, that if a body, in any state of electricity, be in the neighbourhood of an overcharged or undercharged body A, while it communicates with C by a canal leading from the side most remote from A, fluid will be driven from B into C, or abstracted from C into B.

107. It is not, however, so clear, that when the canal leads from the side nearest to A (as in fig. 23.), fluid will be driven from B into C. We conceive the fluid to be moveable in the body and in this canal, but not to escape from it. Its motion, therefore, in this case, should, in the opinion of Mr. Cavendish, resemble the running of water in a syphon by the pressure of the air. While the repulsion of the redundant fluid in A allows the bend of the syphon nearest to A to retain fluid, a current should take place from B along the short leg, in consequence of the superior action on the fluid in the long leg. But if the repulsion of A can drive the fluid out of the bend between B and F, Mr. Cavendish thinks, that it does not appear that fluid will come up from B in opposition to the repulsion of A, and then run along to D. But fluid does not move, in either of these cases, on the principle of a syphon; because there is nothing to hinder the fluid from expanding in the part EDF. And we are rather disposed to think, that it will always move from B, over the bend, to C: For even if the fluid can be completely driven out of the bend EF, it must be done by

degrees, and the fluid in the long leg will, from the very beginning of the action of A, be more moved from its place than that in the short leg; and therefore will yield to the compression, which acts transversely, and, by thus yielding more toward F than toward E, the fluid will rush through the contracted part, and go into C. We do not say this with full confidence; but are thus particular, on account of an important use that may be made of the experiment. For if the Body A be undercharged, fluid will certainly be attracted from C, and pass over the bend into B, however great the action of A may be. Perhaps this may be so contrived, therefore, as to decide the long agitated question, *Whether the electricity of excited glass be plus or minus?* If it be found that this apparatus, being presented to the rubber of an electrical machine, diminishes the positive electricity of C, and increases that of B; but that, presenting the same apparatus to the prime conductor, makes little change —we may conclude, that the electricity of the prime conductor is positive. We have tried the experiment, paying attention to every circumstance that seemed likely to insure success; but we have always found hitherto, that the apparatus was equally affected by both electricities.

We must now consider the action of electrified bodies on the canals of communication; because this will give us the easiest method of ascertaining the proportion in which the expelling fluid is distributed between them. For when two bodies communicate by a canal, and have attained a permanent state, we must conceive that their opposite actions on the fluid moveable along this canal are in equilibrio, or are equal. This will generally be a much easier problem than their action on each other, since we have seen a little ago, that the fluid in a slender canal is of uniform density very nearly. A very few examples of the most important of the simple cases must suffice.

108. Therefore let AC *a* (fig. 24.) represent the edge of a thin conducting circular plate, to which the slender canal

CP is perpendicular in the centre. It is required to determine the action of the matter or fluid, uniformly spread over this plate, on the fluid moveable in the canal PC?

109. *First*,—Required the action of a particle in A on the fluid in the whole canal? Join AP; and call CP x, AP y, and AC r; and let f express the intensity of action at the distance 1, or the unit of the scale on which the lines are measured.

The action of A on P, in the direction AP, is $\frac{f}{y^2}$. This when estimated in the direction CD, is reduced to $\frac{f}{y^2} \times \frac{x}{y}$; and is therefore $= f\frac{x}{y^3}$. Therefore the fluxion of the action in the direction CP, on the whole canal is $f\frac{x}{y^3}\dot{x} = f\frac{y\dot{y}}{y^3}$ (because $x : y = \dot{y} : \dot{x}$) $= f \times \frac{\dot{y}}{y^2}$. The variable part of the fluent is $= f\frac{-1}{y}$, and the complete fluent is $= f\left(C - \frac{1}{y}\right)$, where C is a constant quantity, accommodated to the nature of the case. Now, the action must vanish when the canal vanishes, or when $x = 0$, and $y = r$. Therefore $C - \frac{1}{r} = 0$, and $C = \frac{1}{r}$; and the general expression of the action is $f\left(\frac{1}{r} - \frac{1}{y}\right), = f\frac{y-r}{r\,y}$, expressing the action of a particle in the circumference of the plate on the fluid in the whole canal CP.

2. Required the action of the plate, whose diameter is A a, on the particle P?

110. Let a represent the area of a circle, whose radius is $= 1$. Then $a\,r^2$ is the area of the plate, and $2\,a\dot{r}r$ is the fluxion of this area; because $r : y = \dot{y} : \dot{r}$, $2\,a\,r\dot{r}$ is $= 2\,a\,y\,\dot{y}$. Therefore the fluxion of the action of the plate on the

particle P is $f \times 2ayy \times \frac{x}{y^3}, = 2fax \times \frac{y}{y^2}$. The fluent of this has for its variable part $2fax \times \frac{-1}{y}$ (for when the particle P is given, x does not vary). This is $= 2fa \times \frac{-x}{y}$. To complete this fluent, we must add a constant quantity, which shall make the fluent $= 0$ when the particle P is at an infinite distance; and therefore when $x = y$. Therefore $\frac{y}{y} - \frac{x}{y} = 0$, or $1 - \frac{x}{y} = 0$, or $C = 1$; and the complete fluent for the whole plate is $2fa\left(1 - \frac{x}{y}\right)$.

111. The meaning of this expression may not occur to the reader: for $1 - \frac{x}{y}$ is evidently an abstract number; so is a. Therefore the expression appears to have no reference to the size of the plate. But this agrees with the observation in § 91, where it was shewn, that provided the angle of the cone or pyramid remained the same, the magnitude of the base made no change in its attraction or repulsion for a particle in the vertex.

It will appear by and bye, that $1 - \frac{x}{y}$ is a measure or function of a certain angle of a cone.

Cor. If PC be very small in proportion to AC, the action is nearly the same as if the plate were infinite: For when the plate is infinite, $\frac{x}{y}$ is $= 0$, and the action is $= 1$, whatever is the distance (see § 91—93). Therefore, when x is very small in comparison of r, and consequently of y, $1 - \frac{x}{y}$ is very nearly $= 1$.

112. *Third*, To find the action of the plate on the whole column?

The fluxion of this must be $= 2fa \times (1 - \frac{x}{y})\dot{x}$, or $2fa(\dot{x} - \frac{\dot{x}\,x}{y})$, or $2fa \times (\dot{x} - \dot{y})$; because $\dot{y} = \frac{\dot{x}\,x}{y}$. The fluent of this has for its variable part $2fa \times (x - y)$. A constant quantity must be added, which shall make it $= 0$ when the column $= 0$; that is when $y = r$, and $x = 0$; that is, $C - r = 0$, and $C = r$. Therefore the complete fluent is $= 2fa\,(x + r - y)$.

113. Thus have we arrived at a most simple expression of the attraction or repulsion of a plate for such a column, or for portions of such a column. And it is most easily constructed geometrically, so as to give us a sensible image of this action, of easy conception and remembrance. It is as follows: Produce PC till CK = CA, and about the centre P describe the arch AI, cutting CK in I. Then $2fa \times$ IK is evidently the geometrical expression of the attraction or repulsion. This is plainly a cylinder, whose radius is a unit of the scale, and whose height is twice IK.

In like manner, by describing the arch A i round the centre p, we have $2fa \times i$ K for the action of the plate on the small column C p; and $2fa \times$ I i is the action of the plate on the portion P p.

The general meaning of the expression $2fa \times$ IK is, that the action of the whole plate on the column PC is the same as if all the fluid in the cylinder $a \times 2$ IK, were placed at the distance 1 from the acting particle.

From this proposition may be easily deduced some very useful corollaries by the help of the geometrical construction.

114. *First*, If PC be very great in comparison with AC, the action is nearly the same as if the column were infinitely extended; for in this case IK is very nearly = CK, the difference being to the whole nearly as AC to twice AP.

115. *Second*, If, in addition to this last condition, another column p C be very small in comparison of AC, then the

action on PC is to that on p C very nearly as p C to AC. For it will appear that i K : IK $=p$ C : AC very nearly. It is exactly so when CP : CA $=$ CA : C p; and it will always be in a greater proportion than that of p C to IK.

This will be found to be a very important observation.

The redundant fluid has hitherto been supposed to be uniformly spread over the plate : but this cannot be ; because its mutual repulsion will cause it to be denser near the circumference. We have not determined, by a formula of easy application, what will be the variation of density. Therefore let us consider the result of the extreme case, and suppose the whole redundant fluid to be crowded into the circumference of the plate, as we saw that it must be on the surface of a globe.

116. In this case the action on the fluid in the canal will be $f a \left(r - \frac{r^2}{y}\right)$. For the area of the plate is $a r^2$, and the action of a particle in the circumference on the whole canal was shewn (§ 109.) to be $f\left(\frac{y-r}{r y}\right)$. Therefore the action of the whole fluid crowded into the circumference is $f a r^2 \times \frac{y-r}{r y}, = f a r \frac{y-r}{y}$. It may be represented as follows : Describe the quadrant C b BE, cutting AP and A p in B and b. Draw BD and b d parallel to PC. Then PB $= y - r$, and DC $= r \frac{y-r}{y}$. Therefore the action is represented by f multiplying a cylinder, whose radius is 1 and height is DC. In like manner, d C is the height of the cylinder corresponding to the column pC, and D d the height corresponding to P p.

117. *Cor.* 1. When CP is very great in comparison with CA, the point D is very near to A, and I is very near to C, and CD is to IK nearly in the ratio of equality. In this case the action of the fluid, uniformly spread over the plate, is nearly double of the action of the same fluid crowd-

ed round the circumference; for they are as cylinders having the same bases and heights in the ratio of 2 IK to DC, which is nearly the ratio of 2 to 1.

118.—2. On the other hand, when the column p C is very short, the action of the fluid spread uniformly over the plate is to its action, when crowded round the circumference, nearly in the ratio of 4 AC to pC. For these actions are in the ratio of $2fa \times i$ K to $1fa \times d$ C, or as $2i$ K to d C, or nearly as $2p$ C to d C, or more nearly as $2bd$ to d C. But $Cd : bd = bd : bA + Ad$, or nearly $= bd : 2$ CA. Therefore $Cd : 2bd = p$C : 4 CA nearly.

119. Hence we see that the action on short columns is much more diminished by the recess of the redundant fluid toward the circumference than that on long columns. Therefore, any external electric force which tends to send fluid along this canal, and from thence to spread it over the plate, will send into the plate a greater quantity of fluid than if the fluid remained ultimately in a state of uniform distribution over its surface; and the odds will be greater when the canal is short.

120. *Lastly*, on this subject, if KL be taken equal to AP, or PL be equal to KI, the repulsion which all the fluid in the plate, collected in K, would exert on the fluid in the canal CL, is equal to the repulsion which the same fluid, constipated in the circumference, would exert on the column CP. For we have seen that the action of a particle in A on the whole column PC, when estimated in the direction PC, is $\frac{y-r}{yr}$; and it is well known that the action of a particle in K for the column CL is $\frac{1}{KC} - \frac{1}{KL}$, or $\frac{1}{r} - \frac{1}{y}, = \frac{y-r}{yr}$. Therefore the action of the whole fluid, collected in the circumference, on the column CP is equal to that of the same fluid, collected in K, on the column CL.

121. *Cor.* 1. If the column CP is very long in proportion to AC or KC, the actions of the fluids in these two different

situations are very nearly the same. The action of the fluid collected in K exceeds its action when collected in A only by its action on the small and remote column LP. The action of all the fluid collected at K on the column CP is easily had by taking C l = KP. It is equal to the action of the same fluid placed in A on the column C l.

122. *Cor.* 2. The action of all the fluid uniformly spread, exerted on the column CP, is to the action of the same fluid collected in K, exerted on the column CL, as 2 IK to CD.

123. If the column CP is very great in proportion to AC, the half breadth of the plate, the action in the first case is very nearly double of the action in the other case, and is exactly in this proportion if CP is of infinite extent.

124. *Cor.* 3. If CNO be a spherical surface or shell of the same thickness and diameter as the plate A a, and containing redundant fluid of the same uniform density, the action of this fluid on the column CL is double of the action of the fluid uniformly spread over the plate on the column CP, and quadruple of the action of the fluid collected in the circumference: for the action is the same as if all were collected in the centre K, and the surface of the sphere is four times that of the plate, and therefore they are as IK to 2 CD.

Let us now consider the comparative actions of different plates or spheres on the canals.

125. If two circular plates, DE, de (plate II. fig. 1.), or two spherical shells, ABO, $a\,b\,o$, of equal diameters and thickness with the plates, and containing redundant fluid of equal density, communicate with infinitely extended straight canals OP, $o\,p$, passing through their centres perpendicular to their surfaces, also containing fluid uniformly distributed and of equal density—the repulsions will be as the diameters. For the repulsion of the spherical surfaces is the same as if all the fluid were collected at their centres; and the repulsion of the fluid uniformly spread over the surfaces of the plates is double of its repulsion if collected at the centres of

these spheres; it follows, that the repulsions of the plates are proportional to those of the spheres. But because the repulsion of a plate whose radius is r was shewn to be $= 2a \times \overline{r + x - y}$, and when the column is infinitely extended, x is equal to y, and $\overline{r + x - y} = r$, it follows, that the repulsions of the plates are as $2a \times R$ and $2a \times r$, or proportional to their diameters. Therefore the repulsions of the spheres are in the same proportion.

126. *Cor.* 1. If the canals are very long in proportion to the diameters of the plates or spheres, the repulsions are nearly in the same proportion.

127. *Cor.* 2. But as the lengths of the canals diminish, the repulsions approach to equality; for it was shewn, that when the canal was very small, the repulsion was to that for an infinite column as the length of the canal to the radius of the plate. Therefore if the radius of the greater plate be (for example) double of that of the smaller, and the little column be $\frac{1}{20}$th of the radius, it will be $\frac{1}{10}$th of the radius of the smaller plate. Now $\frac{1}{10}$th of half the repulsion is equal to $\frac{1}{20}$th of the double repulsion. Also, in the case of the spheres, the repulsion of a particle at the surface is as the quantity of fluid directly, and as the square of the radius inversely; but when the density is the same in both shells, the quantity is as the surface, or as the square of the radius. Therefore the repulsions are equal.

128. *Cor.* 3. If the density of the fluid in two spherical shells be inversely as the diameters, the repulsions for an infinitely extended column of fluid are equal; for each repels as if all the fluid was collected in the centre. Therefore if the density, and consequently the quantity, be varied in any proportion, the repulsion will vary in the same proportion. The repulsions will now be as $CO \times \frac{1}{CO}$ to $co \times \frac{1}{co}$, or in the ratio of equality.

129. *Cor.* 4. When the quantities of redundant fluid in two spheres are proportional to their diameters, their repul-

sions for an infinitely extended canal are equal: for if this redundant fluid is constipated in the surfaces of the spheres, as it always will be when they consist of conducting matter, the densities are as the diameters inversely, because the surfaces are as the squares of the diameters. Therefore, by the last corollary, their actions on an infinitely extended canal are equal. But in spheres of nonconducting matter it may be differently disposed, in concentric shells of uniform density. This makes no change in the action on the fluid that is without the sphere, because each shell acts on it as if it were all collected in the centre. Therefore the repulsions are still equal.

130. *Cor.* 5. Two overcharged spheres, or spherical shells, OAB, *o a b* (plate II. fig. 2), communicating by an infinitely extended canal of conducting matter, contain quantities of redundant fluid proportional to their diameters; for their actions on the fluid in the interjacent canal must be in equilibrio, and therefore equal. This will be the case only when the quantities of fluid are in the proportion of their diameters.

When the canals are very long in proportion to the diameters of the spheres, the proportion of the quantities of redundant fluid will not greatly differ from that of the diameters.

131. *Cor.* 6. When the spheres of conducting matter are thus in equilibrio, the pressures of the fluid on their surfaces are inversely as their diameters; for the repulsion of a particle at the surface is the same with the tendency of that particle from the centre of the sphere, the actions being mutual. Now this is proportional to the quantity of redundant fluid directly, and to the square of the distance from the centre inversely, that is, to the diameter directly, and to the square of the diameter inversely, that is, to the diameter inversely.

Hence it follows, that the tendency to escape from the spheres is inversely as the diameter, all other circumstances

being the same: for in as far as the escape proceeds from mere electric repulsion, it must follow this proportion. But there are evident proofs of the co-operation of other physical causes. We observe chemical compositions and decompositions accompanying the escape of electric fluid, and its influx into bodies: we are ignorant how far, and in what manner, these operations are affected by distance. Boscovich shews most convincingly, that the action of a particle (of whatever order of composition), on external atoms and particles, is surprisingly changed by a change in the distance and arrangement of its component atoms. A constipation, therefore, to a certain determined degree and lineal magnitude, may be necessary for giving occasion to some of those chemical operations that accompany, and perhaps occasion, the escape of the electric fluid. If this be the case (and it is *demonstrable* to be possible, if the operations of Nature be owing to attractions and repulsions), the escape *must* be desultory. It is actually so; and this confirms the opinion.

The public is indebted to Mr. Cavendish for the preceding theorems on the action of spheres and circular plates. He has given them in a more abstract and general form, applicable to any law of electric action which experience may warrant. We have accommodated them to the inverse duplicate ratio of the distances, as a point sufficiently established; and we hope that we have rendered them more simple and perspicuous. We have availed ourselves of Mr. Coulomb's demonstration of the uniform density in the canal, without which the theorems could not have been demonstrated. The minute quantity of the fluid in the canal can have no sensible effect on the disposition or proportion of the fluid in the plates or spheres.

It may be thought that the last corollary, respecting the equilibrium of two spheres, is not agreeable to hydrostatical principles, which require the equality of the two forces which balance each other at the orifices of the slender cylin-

dric canal; whereas, in that corollary, the forces at the extremities of the canal are inversely as the diameters of the spheres or plates. This would be a valid objection, if the compressing forces acted only on the extremities of the canals; but they act on every particle through their whole length. It is not, therefore, the pressure at one end of the canal that is in equilibrio with the pressure at the other end, by the interposition of the fluid. It is the pressure at one end, together with the sum of all the intermediate pressures in that direction, that is in equilibrio with all the pressure in the opposite direction. The pressures at the ends are only parts of the whole opposite pressures; they are the first in each account. In this manner a slender pipe, having a ball at each end, may be kept filled with mercury, while lying horizontal, if the air in each ball is of equal density. But if it be raised perpendicular to the horizon, it cannot remain filled from end to end, unless the air of the ball below be made so elastic by condensation, that its pressure on the lower orifice of the pipe exceed the pressure of the air in the upper ball on the other orifice by a force equal to the weight of the mercury, that is, to the aggregate of the action of gravity on each particle of mercury in the pipe. Therefore the repulsions of the spheres that we are speaking of are in equilibrio by the intervention of the fluid in the canal, in perfect consistency with the laws of hydrostatical pressure.

Mr. Cavendish has pursued this subject much farther, and has considered the mutual action of more than two bodies, communicating with each other by canals of moveable fluid uniformly dense. But as we have not room for the whole of his valuable propositions, we selected those which were elementary and leading theorems, or such as will enable us to explain the most important phenomena. They are also such, as that the attentive reader will find no difficulty in the investigation of those which we have omitted.

Mr. Cavendish's most general proposition is as follows:

182. When an overcharged body communicates, by a canal of *very great* length, straight or crooked, with two or more similar bodies, also at a very great distance from each other, and all are in electric equilibrium, and consequently each body overcharged in a certain determined proportion, depending on its magnitude, if any two of these bodies are made to communicate in the same manner, their degrees of electricity are such, that no fluid will pass from one to the other, their mutual actions on the fluid in this canal being also in equilibrio. He brings out this by induction and combination of the single cases, each of which he demonstrates by means of the following theorem:

133. The action of an overcharged sphere ACB (Plate II. fig. 1.) on the fluid in the whole of a canal df P that is oblique, tending to impel the fluid in the direction of that canal, is equal to its action on the fluid in the whole of the rectilineal canal CP. Let hi be a minute portion of the straight canal, and fd the portion of the crooked canal which is equidistant from the centre C of the sphere; draw the radii Cf, Cd, and the concentric arches hf, id, the latter cutting fC in g; and draw ge perpendicular to fd; the force acting on ih, impelling it toward P, may be represented by hi. The same force acting on df, in the direction cf, must therefore be expressed by gf. This, when estimated in the direction of the canal df, is reduced to ef; but it is exerted on each particle of df. Now $df : gf = gf : ef$, and $df \times ef = gf^2$, $= gf \times hi$; therefore the whole force on df, in the direction df, is equal to the force on ih, in the direction ih. Hence the truth of the proposition is manifest.

We beg the curious reader to apply this to the case in hand, and he will find, that the most complicated cases may all be reduced to the simple ones which we have demonstrated to be strictly true when the bodies are spheres or plates, and the canals infinitely long, and which are very nearly true when the canals are *very long*, and the bodies similar: And

we now proceed to one compound case more, which includes all the most remarkable phenomena of electricity.

134. Let HK, AB, DF, and LM (plate II. fig. 3.), be four parallel and equal circular plates, two of which, HK and AB, communicate by a canal GC of indefinite extent, joining their centres, and perpendicular to their planes; let DF and LM be connected in the same manner, and let the two canals be in one straight line; let the plate HK be overcharged, and the plate LM just saturated. It is required to determine the disposition and proportion of the electric fluid in the plates which will make this condition of HK and LM possible and permanent, every thing being in equilibrio?

The plate HK being overcharged, and communicating with AB, AB must be overcharged in the same manner, and being also equal to HK, it must be overcharged in the same degree, containing an equal quantity of redundant fluid disposed in the same manner. To simplify the investigation, we shall first suppose that the redundant fluid is uniformly spread over the surfaces of both.

When the plates HK and AB are in this state, let the plates DF and LM be brought near them, as is represented in the figure, CE being the distance of the centres of AB and DF. It is evident, that the redundant fluid in AB will act on the natural moveable fluid in DF, and drive some of it along the canal EN, and render LM overcharged. Take off this redundant fluid in LM. This will diminish or annihilate the repulsion which it was beginning to exert on the canal EN; therefore more fluid will come out of DF, and again render LM overcharged. The redundant fluid in LM may again be taken off, in less quantity than before, as is plain. Do this repeatedly till no more can be taken off. But this will undoubtedly render DF undercharged, and it will now contain redundant matter. This will act on the fluid in the canal GC, and abstract it from G; therefore fluid will come out of HK into AB. HK will be less

overcharged than before, and AB will be more overcharged. But the now increased quantity of redundant fluid in AB will act more strongly on the moveable fluid in DF, and drive more out of it. This will leave more redundant matter in it than before, and this will act as before on the fluid in the canal GC. This will go on, by repeatedly touching LM, till at last all is in equilibrio. Or this ultimate state may be produced at once by allowing LM to communicate with the ground. And now, in this permanent state of things, HK contains a certain quantity of redundant fluid; AB contains a greater quantity; DF contains redundant matter; and LM contains its natural quantity. The demand of the problem therefore is to determine the proportion of the redundant fluid in HK to that in AB, and the proportion of the redundant fluid in AB to the deficiency of fluid in DF. The dynamical considerations which determine these proportions are, 1*st*, The repulsion of the redundant fluid in AB, for the fluid in the canal EN, must be precisely equal to the attraction of the redundant matter in DF for the same fluid in the canal; for LM, being saturated, is neutral. 2*d*, The repulsion of the redundant fluid in HK, for the whole fluid in the canal GC, must balance the excess of the repulsion of the redundant fluid in AB above the attraction of the redundant matter in DF for the same.

Let the redundant fluid in AB be $= f$.
the redundant matter in DF $= m$.
the redundant fluid in HK $= \mathrm{F}$.

Because HK and AB are equal, there can be no doubt but that the fluid in those plates would be similarly disposed; and it is highly probable, that if AB be very near DF, the redundant fluid in AB, and the redundant matter in DF, will also be disposed nearly in the same manner. This will appear plainly when we consider with attention the forces acting between a very small portion of AB and the corresponding portion of DF. The probability that this is the case is so evident, that we apprehend it unneces-

sary to detail the proofs. We shall afterwards consider some circumstances which shew that the disposition in the three plates will (though nearly similar) be nearer to a state of uniform distribution than if only AB and HK had been in action. Assuming therefore this similarity of distribution, it follows, that their actions on the fluid in the canals will be similar, and nearly proportional to their quantities.

Therefore let 1 be to n as the repulsion of the fluid in AB, for the fluid that would occupy CE, is to its repulsion for the fluid in CG.

Then the action of AB on EN is $f \times \overline{n-1}$, and the action of DF on EN is $m\,n$; therefore, because the plate LM is inactive, the actions of AB and DF on EN must balance each other, and $f \times \overline{n-1} = m\,n$, and $m = f \times \frac{n-1}{n}$.

The repulsion of f for the fluid in CG is $f\,n$. The attraction of m for it is $m \times \overline{n-1}$; and because $m = f \times \frac{n-1}{n}$, the attraction of m for the fluid in CG is $f \times \frac{\overline{n-1}}{n} \times \overline{n-1}$. Therefore the repulsion of f is to the attraction m as $f\,n$ to $f \times \frac{\overline{n-1}^2}{n}$, or as $f\,n^2$ to $f \times \overline{n-1}^2$, or as n^2 to $\overline{n-1}^2$ Call the repulsion of $f\,(r)$, and the attraction of $m\,(a)$. We have $r : a = n^2 : \overline{n-1}^2$

and $\quad r : r - a = n^2 : n^2 - (n-1)^2 = n^2 : 2\,n - 1.$

Therefore, because the repulsion of F is equal to this excess of r above a, we have $n^2 : 2\,n - 1 = f$: F, and $\mathrm{F} = f \frac{2\,n-1}{n^2}$, or $f = \mathrm{F} \frac{n^2}{2\,n-1}$. Therefore, if n^2 is much greater than $2\,n - 1$, the quantity of redundant fluid in AB will be much greater than the quantity in HK.

135. Now when the electric action is inversely as the square of the distance, and EC is very small in comparison with AC, we have seen (§ 115.) that $1 : n$ nearly $=$ CE : CA, or that n is nearly $\frac{\mathrm{AC}}{\mathrm{E}}$. When this is the case, and conse-

quently n is a considerable number, we may take the number $\frac{n^2}{2n}$ for $\frac{n^2}{2n-1}$ without any great error. In this case f is equal to $F \times \frac{n}{2}$ very nearly. Suppose CA to be six inches, and CE to be $\frac{1}{20}$th of an inch; this will give $n = 120$, and $f = 60$ F; or, more exactly, $f = F \times \frac{n^2}{2n-1} = \frac{14{,}400}{239}$; $= 60\frac{1}{4}$ F. If, instead of the plate HK, we employ a globe of the same diameter, f will be but half of this quantity, or $f = F \times \frac{n}{4}$ (§ 123, 124.)

136. It also appears, that when the plates AB and DF are very near to each other, and consequently n a large number, the deficiency in DF is very nearly equal to the redundancy in AB. In the example now given, $m = \frac{119}{120} f$ being $= f \times \frac{n-1}{n}$.

137. Yet this great deficiency in DF does not make it electrical on the side toward LM. It is just so much evacuated, that a particle of fluid at its surface has no tendency to enter or to quit it.

Lastly, this great quantity of fluid collected in AB does not render it more electrical than HK.

In general, things are in the condition treated of in § 22, 23, &c.

The attentive reader will readily see, that this account of the apparatus of four plates is only an approximation to the condition that really obtains under our observation. Our canals are not of indefinite length, nor occupied by fluid that is distributed with perfect uniformity; nor is the fluid uniformly spread over the surface of the plates. He will also see, that the real state of things, as they occur in our experiments, tends to diminish the great disproportion which this imaginary statement determines. But when the canals are very long in comparison with the diameters of the plates,

and AB is very near to DF, the difference from this determination is inconsiderable. We shall note these differences when we consider the remarkable phenomena that are explained by them.

In the mean time, we shall just mention some simple consequences of the present combination of plates.

138. Suppose AB touched by a body. Electric fluid will be communicated; but by no means all the redundant fluid contained in AB: only as much will quit it as will reduce it to a neutral state, if the body which touches it communicates with the ground; that is, till the attraction of the redundant matter in DF attracts fluid on the remote side of AB as much as the redundant fluid left in AB repels it. When this has been done, DF is no longer neutral; for the repulsion of AB for the fluid in EN is now diminished, and therefore the attraction of DF will prevail. If we now touch DF, it may again become neutral with respect to EN; but AB will now repel again the fluid in CG, and again be electric on that side by redundancy. Touching AB a second time, takes more fluid from it, and DF again becomes electric by deficiency, and attracts fluid on that side.—And thus, by repeatedly touching AB and DF alternately, the great accumulation of fluid in AB may be exhausted, and the nearly equal deficiency in DF may be made up.

139. But this may be done in a much more expeditious way. Suppose a slender conducting canal *a b d* brought very near to the outsides of the plates, the end *a* being near to A, and the end *d* to D. The vicinity of *a* to A causes the fluid in *a b* to recede a little from *a* by the repulsion of the redundant fluid in AB. This will leave redundant matter in *a*, which will strongly attract the redundant fluid from A, and *a* may receive a spark. But the consequence, even of a nearer approach of the fluid to the outward surface of A, will render the corresponding part of DF more attractive, and the retiring of fluid from *a* along *a b* will push

some of its natural fluid toward *d;* and thus A becomes more disposed to give out, and *a* to take in, while *d* is disposed to emit, and D to attract. Thus every circumstance favours the passage of the whole, or almost the whole, redundant fluid to quit AB at A, to go along *a b d*, and to enter into DF at D.

140. It is plain, that there must be a strong tendency in the fluid in AB to go into DF, and that the plates must strongly attract each other. A particle of fluid situated between them tends toward DF with a force, which is to the sole repulsion of AB nearly as twice the redundant fluid in it to what it would contain if electrified to the same degree while standing alone.

With this particular and remarkable case of induced electricity, we shall conclude our explanation of Mr. Æpinus' Theory of Electric Attraction and Repulsion. The reader will recollect, that we began the consideration of the disposition of the electric fluid in bodies, in order to deduce such legitimate consequences of the hypothetical law of action as we could compare with the phenomena.

141. These comparisons are abundantly supplied by the preceding paragraphs, particularly by § 74, 75, 76; by § 130, and by § 134.

Let a smooth metal sphere be electrified positively in any manner whatever, and then touch it with a small one in its natural state. The redundant fluid is divided between them in a proportion which the theory determines with accuracy. By the theory also the redundant fluid in both acts as if collected in the centre. Therefore the proportion of the repulsions is determined. These can be examined by our electrometer. But, as this mensuration may be said to depend on the truth of the theory, we may examine this independent of it. Let the balls be equal. Then the redundant fluid is divided equally between the bodies, whatever be the law of action. Therefore observe the electrometer, as it is af-

fected by the electrified body, both before and after the communication. This will give the positions of the electrometer which correspond to the quantities 1 and $\frac{1}{2}$.

142. Take off the electricity of one of the balls by touching it, and then touch the other ball with it. This will reduce to $\frac{1}{2}$ of itself the quantity $\frac{1}{2}$, and therefore to $\frac{1}{4}$th of the original quantity. This will determine the value of another position of the electrometer. In like manner, we obtain $\frac{1}{8}$th, $\frac{1}{16}$th, &c. &c. Then, by touching a ball containing 1 with a ball containing $\frac{1}{2}$ we get a position for $\frac{3}{4}$, $\frac{3}{8}$, &c. Proceeding in this way, we graduate our electrometer independently of all theory, and can now examine the electricity of bodies with confidence. The writer of this article took this method of examining his electrometer, not having then seen Mr. Cavendish's dissertation, which gives another mode of measurement. He had the satisfaction of observing, in the first place, that the positions of the instrument which unquestionably indicated 1, $\frac{1}{4}$, $\frac{1}{9}$, &c. were precisely those which should indicate them if electric repulsion be inversely as the squares of the distances. Having thus examined the electrometer, it was easy to give to balls any proposed degree of electricity, and then make a communication between balls of very different diameters. The electrometer informed him when the repeated abstractions by a small ball reduced the electricity of a large ball to $\frac{1}{2}$, $\frac{1}{4}$, &c. This shewed the proportion of electricity contained in balls of different diameters. This was also found to be such as resulted from an action in the inverse duplicate ratio of the distances.

143. Long after this, Mr. Cavendish's investigation pointed out the proportion of the redundant electric fluid in balls of different sizes joined by long wires; in § 130, &c. these were examined—and found to be such as were so indicated by the electrometer.

144. And, lastly, the mode of accumulating great quantities of fluid by means of parallel plates, gave a third way

of confronting the hypothetical law with experiment. The argument was no less satisfactory in this case; but the examination required attention to particulars not yet mentioned, which made the proportions between the fluid in HK and AB (fig. 3.) widely different from those mentioned in the preceding paragraphs. These circumstances are among the most curious and important in the whole study, and will be considered in their place.

145. We rest therefore with confidence on the truth of the law of electric action, assumed by us as a principle of explanation and investigation. It is quite needless and unprofitable to give any detail of the numerous experiments in which we confronted it with the phenomena. The scrupulous reader will get ample satisfaction from the excellent experiments of Mr. Coulomb with his delicate electrometer. He will find them in the Memoirs of the Academy of Sciences of Paris for 1784, 1785, 1786, and 1787. Some of them are of the same kind with those employed by the writer of this article; others are of a different kind; and many are directed to another object, extremely curious and important in this study, namely, to discover how the electric fluid is disposed in bodies; and a third set are directed to an examination of the manner in which the electric fluid is dissipated along imperfect conductors.

But we have already drawn this article to a great length, and must bring it to an end, by explaining some very remarkable phenomena, namely, the operation of the Leyden phial, the operation of the electrophorus, and the dissipation of electricity by sharp points and by imperfect conductors.

146. The observations of Mr. Watson on the necessity of connecting the rubber of an electrical machine with the ground, might have suggested to philosophers the doctrine of *plus* and *minus* electricity, especially after the valuable discoveries of Mr. Symmer and Cigna. A serious consideration of these general facts would have led to the theory

of coated glass almost at its first appearance. But the historical fact was otherwise; and a considerable time elapsed between the first experiments with charged glass by Kleist, and the clear and satisfactory account given by Dr. Franklin, of all the essential parts of the apparatus, and the probable procedure of nature in the phenomenon. The impermeability of glass by the electric fluid, and the consequent abstraction of it from the one side while it was accumulated on the other, suggested to his acute mind the leading principle of electrical philosophy; namely, that all the phenomena arise from the redundancy or deficiency of electric fluid, and that a certain quantity of it resides naturally in all bodies in a state of uniform distribution, and, in this state, produces no sensible effect. This was, in his hands, the inlet to the whole science; and the greatest part of what has been since added is a more distinct explanation how the redundancy or deficiency of electric fluid produces the observed phenomena. Dr. Franklin deduced this leading principle from observing, that as fast as one side of a glass plate was electrified positively, the other side appeared negative, and that, unless the electricity of that side was communicated to other bodies, the other side could be no farther electrified. Having formed this opinion, the old observations of Watson, Symmer, and Cigna, were explained at once, and the explanation of the Leyden phial would have come in course. It is for these reasons, as much as for the important discovery of the sameness of electricity and of thunder, that Dr. Franklin stands so high in the rank of philosophers, and is justly considered as the author of this department of natural science. Whatever credit may be due to the chemical speculations of De Luc, Wilcke, Winkler, and many others, who have attempted to associate electricity with other operations of nature, by resolving the electric fluid into its constituent parts, all their explanations presuppose a mathematical and mechanical doctrine concerning the mode of action of the ingredients, which will

either account for the total inactivity of the compound, or which will explain, in the very same manner, the action of the compound itself: yet all seem to content themselves with a vague and indistinct notion of this preliminary step, and have allowed themselves to speak of electrical atmospheres, and spheres of activity, and such other creatures of the mind, without once taking the trouble of considering whether those assumptions afforded any real explanation. How different was Newton's conduct. When he discovered that the planets attracted each other in the inverse duplicate ratio of the distances, and that terrestrial gravity was an instance of the same force, and that *therefore* the deflection of the earth was the effect of the accumulated weight of all its parts; he did not rashly affirm this of the planets, till he examined what would be the effect of the accumulated attraction in the above mentioned proportion.

147. Mr. Æpinus has the honour of first treading in the steps of our illustrious countryman; and he has done it with singular success in the explanation of the phenomena of attraction and repulsion, as we have already seen. In no part of the study has his success been so conspicuous as in the explanation of the curious and important phenomena of the Leyden phial. It only remained for him to account for the accumulation of such a prodigious quantity of this agent as was competent to the production of effects which seemed to exceed the similar effects in other cases, out of all proportion. Indeed, the disproportion is so great, as to make them appear to be of a different and incomparable nature. Dr. Wilson's experiments in the pantheon are therefore precious, by shewing that nothing was wanted for the production of all the effects of the Leyden phial but a surface sufficiently extensive for containing a vast quantity of fluid, and so perfectly conducting as to admit of its simultaneous and rapid transference. Therefore we assert, that one of the chief merits of Mr. Æpinus' theory is the satisfactory explanation of the accumulation of this vast quanti-

ty of fluid in a small space. We trust, therefore, that our readers will peruse it with pleasure. But we must here observe, that Mr. Æpinus has not expressly done this in the work which we have already made so much use of, nor in any other that we know of. He has gone no farther than to point out to the mathematicians, that his hypothesis is adequate to the accounting for any degree of accumulation whatever. This he does in that part of his work which contains the formulæ of § 38, 39, 40, 41, &c. And he afterwards shews, that all the phenomena of attraction and repulsion which are observed in the charged jar are precisely such as are necessary consequences of his theory.

148. It is to the Hon. Mr. Cavendish that we are indebted for the satisfactory, the complete (and we may call it *the popular*), explanation of all the phenomena. Forming to himself the same notion of the mechanical properties of the electric fluid with Mr. Æpinus, he examined, with the patience, and much of the address of a Newton, the action of such a fluid on the fluid around it, and the sensible effects on the bodies in which it resided; the disposition of it in a considerable variety of cases: and particularly its action on the fluid contained in slender canals and in parellel plates —till he arrived at a situation of things similar to the Leyden phial. And he then pointed out the precise degree of accumulation that was attainable, on different suppositions concerning the law of electric action in general. We have given an abstract of this investigation accommodated to the inverse duplicate ratio of the distances.

149. From this it appears (§ 135.), that whatever quantity of electric fluid we can put into a circular plate 12 inches in diameter, by simple communication with the prime conductor of an electrical machine, we can accumulate 60 times as much in it by bringing the plate within $\frac{1}{175}$th of an inch of another equal plate which communicates with the ground; and it appears in § 139, that all this accumulated fluid may be transferred in an instant to the other plate (which is

shewn to be almost equally deprived of fluid), by connecting the two plates by a small wire.

But as it was also shewn in that paragraph, that the force with which the accumulated fluid was attracted by the redundant matter in the other plate was exceedingly great, and consequently its tendency to escape was proportionably increased; this accumulation cannot be obtained unless we can prevent this spontaneous transference.

150. Here the non-conducting power of idio-electrics, without any diminution of the action of the electric fluid on fluid or matter on the other side of them, comes to our aid, and we at once think of interposing a plate of glass, or wax, or rosin, or any other electric, between our conducting plates. Such is the immediate suggestion of a person's mind who entertains the Æpinian notion of the electric fluid; and such, we are convinced, is the thought of all who imagine that they understand the phenomena of the Leyden phial. But those who attempt to explain electric action by means of what they call electric atmospheres of variable density or intensity, are not entitled to make any such inference, nor to expect any such phenomena as the Leyden phial exhibits. Electricity, they say, acts by the intervention of atmospheres: Therefore, whatever allows the propagation of this action (conceive it in any manner whatever), allows the propagation of these agents; and whatever does not conduct electric action, does not conduct the agents. Interposed glass should therefore prevent all action on the other plate. This is true, even although it were possible (which we think it is not) to form a clear notion of the free passage of this material atmosphere in an instant, and this without any diminution of its quantity, and consequently of its action, by the displacement of so much of it by the solid matter of the body which it penetrates. Yet without this undiminished action of the electric plate on the fluid, and on the matter, beyond the glass, and on the canal by which its fluid may be driven off into the general mass—no such accumulation

can take place; and if the phenomena of the Leyden phia are agreeable to the results of the Æpinian hypothesis, al explanation by atmospheres must be abandoned. Indeed when the partisans of the atmospheres attempt to explai their conceptions of them, they do not appear to differ fron what are called *spheres of activity* (a phrase first used by Dr Gilbert of Colchester, in his celebrated work *De Magnete e Corporibus Magneticis*): and spheres of activity will be found nothing more than a figurative expression of some indistinct conception of *action in every direction.* When we use the words *attraction* and *repulsion*, we do not speak a whit more figuratively than when we use the general word *action.* These terms are all figurative, only *attraction* and *repulsion* have the advantage of specifying the *direction* in which we conceive the *action* to be exerted.

It therefore becomes still more interesting to the philosopher to compare the phenomena of CHARGED GLASS with the Æpinian theory. They afford an *experimentum crucis* in the question about electric atmospheres.

151. Let G (Plate II. fig. 4.) represent the end of a prime conductor, furnished with Henley's electrometer. Let AB represent a round plate of tinfoil, pasted on a pane of glass which exceeds the tinfoil about two inches all round. The pane is fixed in a wooden foot, that it may stand upright and be shifted to any distance from the conductor. DF represents another plate of the same dimensions as AB, in the centre of which is a wire EN, having a small ball on the end N, to which is attached a Canton's electrometer. This wire passes through the wooden ball O, fastened to the insulating stand P. The glass pane must be very clean, dry and warm. Connect the conductor G with AB by a wire reaching to the centre C. Turn the cylinder of the electrical machine slowly, till the electrometer rise to 30° or 40°, and note the number of turns. Take off the electricity, and having taken away the connecting wire GC, turn the machine again till the electrometer rise to the same height. The

difference in the number of turns will give some notion of the expenditure of fluid necessary for electrifying the plate of tinfoil alone This will be found to be very trifling when the electricity is in so moderate a degree. It is proper, however, to keep to this moderate degree of electrification, because when it is much higher, the dissipation from the edges of the plate is very great. Replace the wire, and again raise the electrometer to 30°. Now bring forward the plate DF, keeping it duly opposite and parallel to AB, and taking care not to touch it. It will produce no sensible change on the position of the electrometer till it come within four or three inches of the glass pane; and even when we bring it much nearer (if a spark do not fly from the glass pane to DF), the electrometer HG will sink but two or three degrees, and the electrometer at N will be little affected. Now remove the plate DF again to the distance of two or three feet, and attach to its ball N a bit of chain, or silver or gold thread, which will trail on the table. Again, raise the electrometer to 30°, and bring DF gradually forward to AB. The electrometer HG will gradually fall down, but will rise to its former height, if DF be withdrawn to its first situation. It is scarcely necessary to shew the conformity of this to the theory contained in § 134, 135, &c. As the plate DF approaches, the redundant fluid in AB acts on the fluid in DF, and drives it to the remote end of the wire EN, as was shewn by the divergency of the balls at N; and then an accumulation begins in AB, and the electrometer HG falls, in the same manner as if part of the fluid in the prime conductor were communicated to AB. When DF communicates with the ground, the electrometer at N cannot shew any electricity, but much more fluid is now driven out of DF, in proportion as it is brought nearer to AB. Instead of connecting AB immediately with the prime conductor, let the wire GC have a plate at the end G, of the same dimensions as AB, having an electrometer attached to the side next to AB. Let this apparatus of two

plates be electrified any how, and note the divergency of the electrometer at H, before DF communicating with the ground, is brought near it, and then attend to the changes. We shall find the divergency of this electrometer correspond with the distance of DF very nearly as the theory requires.

152. While the plates AB and DF are near each other, especially when DF communicates with the ground, if we hang a pith-ball between them by a silk thread, it will be strongly attracted by the plate which is nearest to it, whether DF or AB; and having touched it, it will be briskly repelled, and attracted by the glass pane, which will repel it after contact, to be again attracted and repelled by DF; and thus bandied between the plates till all electricity disappear in both, the electrometer attached to H descending gradually all the while.

153. As all these phenomena are more remarkable in proportion as the plates are brought nearer, they are most of all when DF is applied close to the glass pane. And if, in this situation, we take any accurate method for measuring the intensity of the electricity in the plate HG, before the approach of DF, we shall find the diminution, occasioned by its coming into full contact with the pane, considerably greater than what is pointed out in § 135. When we employed plates of 12 inches diameter, pasted on a pane one-fortieth of an inch in thickness, we found the diminution not less than 199 parts of 200; and we found that it required at least 200 times the revolution of the cylinder to raise the electrometer to the same height as before. This comparison is not susceptible of great accuracy, by reason of many circumstances, which will occur to an electrician. But in all the trials we have made, we are certain that the accumulation greatly exceeded that pointed out by the Æpinian theory as improved by Mr. Cavendish. And we must here observe, that we found this superiority more remarkable in some kinds of glass than others, and more remarkable in

some other idio-electrics. We think that in general it was most remarkable in the coarse kinds of glass, provided they were uniformly transparent. We found it most remarkable in some common glass which had exfoliated greatly by the weather: but we also found that such glasses were very apt to be burst by the charge. The hardest and best London crown-glass seemed to accumulate less than any other; and a coloured glass, which when viewed by reflection seemed quite opake, but appeared brown by transmitted light, admitted an accumulation *greatly* exceeding all that we have tried; but it could not be charged much higher without the certainty of being burst. This diversity in the accumulation, which may be made in different kinds of glass, hinders us from comparing the *absolute* accumulations assigned by the theory with those which experiment gives us. But though we cannot make this comparison, we can make others which are equally satisfactory. We can discover what proportion there is between the accumulation in glass of the same kind, as it may differ in thickness and in extent of surface. Using mirror glass, which is of uniform and measurable thickness, and very flat plates, which come into accurate or equable contact—we found that the accumulation is inversely as the thickness of the plates; but with this exception, that when two plates were used instead of a plate of double thickness, the diminution by the increase of thickness was not nearly in the proportion of this increase. Instead of being reduced to one-half, it was more than two-thirds; and in the kind called Dutch plate, the diminution was inconsiderable.

154. The experiments with the Dutch and other double plates, suggested another instructive and pretty experiment. Observing these plates to cohere with considerable force, it was thought worth while to measure it; which was attempted in this manner: Two very flat brass plates AB, DF (Plate II. fig. 5.), furnished with wires and balls, were suspended, about three inches asunder, by silk threads, as re-

presented in the figure. At G was attached a very fine silver wire, which hung very loose between it and the prime conductor, without coming near the table. Another was attached to N, which touched the table. A plate of mirror glass was set between them, as shewn by QR. When this apparatus was electrified, the threads of suspension immediately began to deviate from the perpendicular, and the plates to approach the glass pane and each other. The pane was carefully shifted, so as to be kept in the middle between them. This result shewed very plainly the pressure of the fluid on one of the plates, and the mutual attraction of the redundant matter and redundant fluid. This increased as the accumulation increased; and it was attempted to compare the attraction with the accumulation, by comparing the deviation of the suspending threads with that of the electrometer attached to the prime conductor; but we could not reconcile the series (which, however, was extremely regular) with the law of electric action. This harmony was probably disturbed by the force employed in raising the silver wires. When more flexible silver threads were used much was lost by dissipation from the roughness of the thread. We did not think of employing a fine flaxen thread moistened: but, indeed, an agreement was hardly to be expected; because theory teaches us, that the distribution of of the redundant fluid in AB will be extremely different from the distribution of the redundant matter in DF, till the plates come very near each other. The accumalation in AB depends greatly on the law of distribution, being less (with any degree of redundancy) when the fluid is denser near the centre of the plate. Other circumstances concurred to disturb this trial; but the theory was abundantly confirmed by the experiment, which shewed the strong attraction arising from the accumulation. This was so great, that although the plates were only three inches in diameter, and the glass pane was $\frac{1}{32}$ of an inch thick, and the threads deviated about 18 degrees from the perpendicular—it requir-

ed above an ounce weight, hung on the wire EN, to separate the plates from the glass.

155. The experienced electrician need not be told, that by bringing the two ends of a bent wire in contact with the two plates (first touching DF with it) discharges the apparatus, and causes the plates to drop off from the pane. But he may farther observe, that if there be attached to each end of the discharging wire a downy feather, and if he first bring the end near the plate DF, and observe the feather to be not at all, or but a very little, affected, and if he then bend round the other end toward the plate AB, *both feathers* will immediately stretch out their fibres to the plates, and cling fast to them, long before the discharging spark is seen. This is a fine proof of the process of discharge, which begins by the induction of electricity on the ends of the discharging wire; first, negative electricity on the end that approaches A, and, in the same instant, opposite electricities at D and the adjoining end of the wire.

156. The following observation of Professor Richmann of St. Petersburgh is extremely instructive and amusing. Let a glass pane be coated on both sides, and furnished with a small electrometer attached to the coatings. It is represented as if seen edgewise in plate II. fig. 6. Let it be charged positively (that is, by redundancy) by the coating AB, while DF communicates with the ground. The electrometer A *a* will stand out from the plate, and D *d* will hang down close by its coating, as long as DF communicates with the ground. But as the electricity gradually dissipates by communication to the contiguous air, the ball *a* will gradually, but very slowly, fall down. We may judge of the intensity of the remaining electricity by the deviation of the electrometer, and we may conceive this deviation divided into degrees, indicating, not angles, but intensities, which we conceive as proportional to the redundancy or deficiency which occasion them.

If we take away the communication with the ground, we shall observe the ball *a* fall down very speedily, and then more slowly, till it reach about half of its first elevation. The ball *d* will at the same time rise to nearly the same height; the angle between the two electrometers continuing nearly the same as at first. When *d* has ceased to rise, both balls will very slowly descend, till the charge is lost by dissipation. If we touch DF during this descent, *d* will immediately fall down, and *a* will as suddenly rise nearly as much; the angle between the electrometers continuing nearly the same. Remove the finger from DF, and *a* will fall, and *d* will rise, to nearly their former places; and the slow descent of both will again continue. The same thing will happen if we touch AB; *a* will fall down close to the plate, and *d* will rise, &c. And this alternate touching of the coatings may be repeated some hundreds of times before the plate be discharged. If we suspend a crooked wire *v m u*, having two pith-balls *v* and *u* from an insulated point *m* above the plate, it will vibrate with great rapidity, the balls striking the coatings alternately; and thus restoring the equilibrium by steps. Each stroke is accompanied by a spark.

All these phenomena are not only consequences of the theory, but their measures agree precisely with the computations deduced from the formulæ in § 22, 23, 24, accommodated to the case by means of § 135, and 136, as we have verified by repeated trials. But it would occupy much room to trace the agreement here, and would fatigue such readers as are not familiarly conversant with fluxionary calculations. The inquisitive reader will get full conviction by perusing Æpinus' Essay, Appendix i. A very distinct notion may be conceived of the whole process, by supposing that in a minute AB loses $\frac{1}{10}$th of the unbalanced redundancy actually in it, and consequently diminishes as much in its action. It will be proved afterwards, that the dissipations in equal times are really in proportion to the super-

ficial repulsions then exerted. We may also suppose, that the action of the redundant fluid, or redundant matter, in either coating, on the external fluid contiguous to it, is to its action on the fluid contiguous to the other coating in the constant proportion of 10 to 9. We select this proportion for the simplicity of the computation. Then the difference of these actions is always $\frac{1}{10}$th of the full action on the fluid contiguous to it. This is also an exact supposition in some particular case, depending on the breadth of the coating and the thickness of the pane.

Now, let the primitive unbalanced repulsion between AB and the contiguous fluid of the electrometer be 100, while DF communicates with the ground. The ball *a* will stand at 100; the ball *d* will hang touching DF. Then *a*, by losing $\frac{1}{10}$th, retains only 90, and would sink to 90°: But as this destroys the equilibrium on the other side, fluid will enter into DF, so as to reduce the deficiency $\frac{1}{10}$th. Therefore nine degrees of fluid will enter; and its action on *a* will be the same as if $\frac{9}{10}$ths of 9, or 8,1 had been restored to AB. Therefore *a* will rise from 90 to 98,1; or it will sink in one minute from 100 to 98,1.

But if we have cut off the communication of DF with the ground, this quantity of fluid cannot come into DF; and the quantity which really comes into it from the air will be to that which escapes from A as the attraction on the side of DF to the repulsion on the side of AB. By the diminution of the repulsion $\frac{1}{10}$th, and the want of 9 degrees of fluid in DF to balance it, DF acquires an attraction for fluid, which may be called nine. Therefore, since $\frac{1}{10}$th of the primitive repulsion of AB has dissipated 10 measures of fluid in the minute, the attraction of DF will cause it to acquire $\frac{1}{10}$th of 9, or 0,9, from the air in the same minute. At the end of the minute, therefore, there remains an unbalanced attraction for fluid =8,1; and consequently an unbalanced repulsion between the redundant matter in DF, and that in the ball *d*. Therefore *d* will rise to 8,1. But

a cannot now be at 98,1; because DF has not acquired 9 measures of fluid, but only $\frac{9}{10}$ths of one measure. Therefore a, instead of rising from 90 to 98,1, will only rise to $90 + \frac{9}{10}$ths $\times \frac{9}{10}$ths; that is, to 90,81.

At the close of the minute, therefore, a is at 90,81 and d is at 8,1, and their distance is 98,91. In the next minute, AB will lose $\frac{1}{10}$th of the remaining unbalanced electricity of that side, and DF will now acquire a greater proportion than before; because its former unbalanced attraction gets an addition equal to $\frac{9}{10}$ths of the loss of AB. This will make a larger compensation in the action on a, and a will not fall so much as before. And because in the succeeding minutes the attraction of DF for fluid is increasing, and the repulsion of AB is diminishing, the compensation in the action on a, by the increased attraction of DF, continues to increase, and the descent of a grows continually slower; consequently a time must come, when the repulsion of AB for fluid is to the attraction of DF for it, nearly in the proportion of 10 to 9. When this state obtains, d will rise no more; because the receipt of fluid by DF, being now $\frac{9}{10}$ths of the loss by AB, it will exactly compensate the additional attraction of DF for fluid, occasioned by that loss. The next loss by AB not being so great, and the next receipt by DF continuing the same, by reason of its undiminished attraction, there will be a greater compensation in the action on a, which will prevent its descending so fast; and there will be more than a compensation for the additional attraction of DF for fluid: that is, the fluid which has now come into DF will render it, and also the ball d, less negative than before; and therefore they will not repel so strongly. Therefore d must now descend. It is evident, that similar reasons will still subsist for the slow descent of a, and the slower descent of d, till all redundancy and deficiency are at an end.

This maximum of the elevation of d happens when a has descended about one half of its elevation; that is, when the

unbalanced repulsion of AB is reduced to about one half. For if one half of the unbalanced fluid be really taken out of AB, and if DF can get no supply whatever, it must acquire an attraction corresponding to $\frac{9}{1}$ ths of this; and if the supply by the air be now opened to it, things will go on in the way already described, till all is discharged.

This account of the process is only an approximation; because we have supposed the changes to happen in a desultory manner, as in the popular way of explaining the acceleration of gravity. The rise of *d* is not at an end till the attraction of DF for fluid is to the repulsion of AB as 19 to 20.

But if we interrupt this progress in any period of it, by touching DF, we immediately render it neutral, and *d* falls quite down, in consequence of receiving a complete supply of fluid. But this must change the state of AB, and cause it to rise $\frac{9}{10}$ths of the desent of *d*. As *a* and *d* were nearly at an equal height before DF was touched, it is plain that *a* will rise to nearly twice its present height; after which, the same series of phenomena will be repeated as soon as the finger is removed from DF.

If, instead of touching DF, we touch AB, the same things must happen; *a* must fall down, and *d* must rise to nearly twice its present height, and all will go on as before, after removing the finger. Lastly, if instead of allowing either side to touch the *ground* alternately, we only touch it with a *small insulated body*, such as the wire with the balls *v* and *u*, the ball attached to the side touched sinks, till the electricity is shared between the coating and the wire with balls. The ball attached to the other coating rises $\frac{9}{19}$ths of the sinking of the first ball. The crooked wire ball is now repelled by the coating which it touched, and the other ball is brought near to the other coating, and must be attracted by it, because the electricities are opposite. This operation evidently tends to transfer the redundant fluid *by degrees* to the side where it is deficient. It needs no explanation. We

shall only mention a thing which we have always observed, without being able to account for it. The vibration of the wire acquires a certain rapidity, which continues for a long while, and suddenly accelerates greatly, and immediately afterwards ceases altogether.

This pretty experiment of Professor Richmann will enable us to understand the operation of the electrophorus, and to see the great mistake of those who say that it is perfectly similar to a discharged glass plate.

157. Thus, then, we see, that all the classes of phenomena, connected with attraction and repulsion, are precisely such as would result from the action of a fluid so constituted. The complete undiminished action of the cause of those phenomena on the other side of the interposed non-conductor of that cause is demonstrated, and all explanation by the mechanical action of material elastic atmospheres of variable density must be abandoned, and the infinitely simpler explanation by the attractive and repulsive forces of the fluid itself must be preferred.

So happily does the Franklinian theory of positive and negative electricity explain the phenomena, when a suitable notion is formed of the manner of action of this fluid. We cannot but think that this is attained, when, to the general doctrine of Æpinus, we add the specification of the law of action, so fully verified by the experiments of Mr. Coulomb, which are in the hands of the public; and are of that simple nature, that any careful experimentor can convince himself of their accuracy (See § 144.) We may therefore proceed with some confidence, and apply this doctrine even to cases where experiment does not offer itself for proof.

158. Dr. Franklin affirms that electric fluid cannot be thrown into one side of the coated pane unless it be abstracted from the other; and that therefore the charged glass contains no more than it did before charging. We indeed find, that we cannot *charge* the inside, if the outside do not communicate with the ground. He proves it also by saying,

that if a person, when insulated, discharges a glass through his own body, he is not found electrified: And he infers, as a necessary consequence of this, that a series of any number of jars may be charged by the same turns of a machine, if we make the outside of the first communicate with the inside of the second, and the outside of the second with the inside of the third; and so on; and the outside of the last communicate with the ground. Having made the trial, and having found that more turns of the machine were necessary, he attributes this to dissipation into the air by the communication. But our theory teaches us otherwise. We learn from it, that the redundant matter in the plate DF is less than the redundant fluid in AB, in the proportion of $n - 1$ to n; and therefore the redundant fluid in the overcharged side of the next plate is no greater. The charge or redundancy in the mth jar of the series will therefore be $\left|\frac{n-1}{n}\right|^m$. Thus, if n, or the charge of the 1st jar, be 60, the charge of the 10th jar will be nearly 51. Although a coated plate cannot be *charged*, unless one of the coatings communicate with the ground, it may be *electrified* as much as one of the coatings can be alone. And this is seen in our attempt to charge it: For as soon as we attempt to electrify one side, the other is electrified also; for it gives a spark, which no unelectrified body will do. Also, when we discharge a jar by an insulated discharger, we always leave it electrical in the same way with the body from which it was charged. If a man is not found electrified after having discharged a jar through his own body, it is owing to the great surface of his body, which reduces the simple electrification of a side of the jar to a very insignificant and insensible quantity.

159. Wilcke (and we believe Franklin before him) maintains, that when the jar has been charged, by connecting one side with the prime conductor and the other with the rubber, it is neutral and inactive on both sides. But this

is not so; and a slight reflection might have convinced them that it cannot be so: if it were, the jar could not be discharged. Each side, while connected with the machine, must be in the condition of the part with which it is connected, and in a disposition to take or give. If the trial be carefully made, it will be found to be *equally active* on both sides; and the discharging rod, having down on its ends, will shew this in an unequivocal manner, and shew that its condition differs in this respect from that of a jar charged in the ordinary way. It is in the maximum state of Richmann's plate, described in § 156, when d rises no more.

160. In discharging a jar A, if instead of the outside communicating with the inside by a wire, we make it communicate with the inside of a second jar B, while the outside of B is made to communicate with the inside of A, we shall find B charged by the discharge of A; and that the discharge of A is not complete, the charge 2^2 always remaining, whatever may have been the magnitude of n.

161. We may infer from this experiment, that when a shock is given to a number of persons, a, b, c, &c. we are not to conclude, that the fluid which comes into the deficient side of the jar is the same which came out of the redundant side. The whole, or perhaps only a part, of the moveable fluid in the person a goes into b, replacing as much as has passed from b into c, &c. Indeed, where the canal is a slender wire, we may grant that great part of the individual particles of fluid which were accumulated on the inside of the jar have gone into the outside. Perhaps the quantity transferred, even in what we call *a very great discharge*, may be but a small proportion of what naturally belongs to a body. This may be the reason why a charge will not melt more than a certain length of wire. Mr. Cavendish ascribes this to the greater obstruction in a longer wire; but this does not appear so probable. A greater obstruction would occasion a longer delay of the transference; and therefore the action of the same quantity would be longer continued.

He proves, that a metal wire conducts many hundred times faster than water; yet, when water is dissipated by a discharge, it is found to have actually conducted a *much greater* proportion of the whole charge. We ascribe it chiefly to this, that, in a short wire, the quantity transferred exceeds the whole quantity belonging to the wire

162. It is surely needless to prove that the theory of the Leyden phial is the same with that of the coated pane. The only difference is, that we are not so able to tell the disposition of the accumulated fluid, and the evacuated matter, in every figure. When the phial is of a globular form, and of uniform thickness, with an exceedingly small neck, we then know the disposition more accurately than in a plate. The redundant fluid is then uniformly distributed. If we could insure the uniformity of thickness, such a phial would be an excellent UNIT for measuring all other charges by; but we can neither insure this (by the manner of working glass), nor measure its want of uniformity: whereas we can have mirror plate made of precisely equal thickness, and measure it. This, therefore, must be taken as our unit.

163. And here we remark, that this gives us the most perfect of all methods for comparing our theory with experiment. We must take two plates, of the same glass and the same thickness, but of different dimensions of coated surface. We must charge both by very long conducting wires on both sides, and then measure how often the charge of the one is contained in the other. Mr. Cavendish has given an unexceptionable method of doing this independent of all theory. As it applies equally to jars, however irregular, we shall take it altogether.

When a jar is charged, observe the electrometer connected with it, and immediately communicate the charge to another equal jar (the perfect equality being previously ascertained by the methods, which will appear immediately). Again note the electrometer. This will give the elevation, which indicates one-half, independent of all theory. Now

electrify a jar, or a row of equal jars, to the same degree with the first, and communicate the charge to a coated mirror plate, discharging the plate after each communication, till the electrometer reaches the degree which indicates one-half. This shews how often the charge of the plate is contained in that of the jar or row of jars.

Let the charge of the plate be to that of the jars as x *to* 1. Then, by each communication, the electricity is diminished in the proportion of $\overline{1+x}$ to 1. If m communications have been made, it will be reduced in the proportion of $\overline{\overline{1+x}}^{m}$ to 1. Therefore $\overline{\overline{1+x}}^{m} = 2$, and $1 + x = \sqrt[m]{2}$, and $x = \sqrt[m]{\overline{2}} - 1$.

When x is small in proportion to 1, we shall be very near the truth, by multiplying the number of communications by 1,444, and subtracting 0,5 from the product. The remainder shews how often the charge of the plate is contained in that of the jars, or $\frac{1}{x}$.

Thus may the perfect equality of two jars be ascertained; and the one which exceeds, on trial, may be reduced to equality by cutting off a little of the coating. An electrician should have a pair of small jars or phials so adjusted. It will serve to discover in a minute or two the mark of one-half electricity for any electrometer, and for any degree; as also for measuring jars, batteries, shocks, &c. much more accurately than any other method: because such phials, constructed as we shall describe immediately, may be made so neutral, and so retentive, that the quantity which dissipates during the handling becomes quite insignificant in proportion to the quantity remaining; whereas, in all experiments with electrometers, constructed with the most curious attention, the dissipations are great in proportion to the whole, and are capricious.

164. It was chiefly by this method that the writer of this article, having read Mr. Cavendish's paper, compared the measures given by experiment with those which result from an action in the inverse duplicate ratio of the distance. When the charges were moderate, the coincidence was perfect; when the charges were great, the large plates contained a little more. This is plainly owing to their being less disposed to dissipate from the edges.

165. We may now follow with some confidence the practical maxims deducible from the theory for the construction of this accumulating apparatus. The theory prescribes a very conducting coating, in close and uninterrupted contact: It prescribes an extensive surface, and a thin plate of idioelectric substance. Accordingly all these are in fact attended by a more powerful effect. Metal is found to be far preferable to water, which was first employed, having been suggested by the original experiments of Gray, Kleist, and Cunæus. A continuous plating is prescribed, in preference to some methods commonly practised; such as filling the jar with brass dust, or gold leaf, or covering its surface with filings stuck on with gum water, or coating the inside with an amalgam of mercury and tin. This last appears, by reflection from the outside, to give a very continuous coating; but if we hold the jar between the eye and the light, we may perceive that it is only like the covering with a cobweb. Yet there are cases where these imperfect coatings only are practicable, and some rare ones where they are preferable. In the medical exhibition of electricity, where the purpose intended is supposed to require the transfusion of a great quantity of the electric fluid, any thing that can diminish the irritating smartness of the spark is desirable. This is greatly effected by those imperfect coatings. Small shocks, which convey the same quantity of fluid with the sharp pungent and alarming spark from a large surface, are quite soft and inoffensive, greatly resembling the spasmodic quivering, sometimes felt in the lip or eye-lid, and will not alarm the most fearful patient.

166. Close contact of the metallic coating is observed to increase the effect of the charge. But it is also found, that it greatly increases the risk of bursting the glass by spontaneous discharge through its substance. An experienced electrician (we think it is Mr Brookes of Norwich) says, that since he has employed paper covered with tinfoil, with the paper next the glass, instead of the foil itself, he has never had a jar burst; whereas the accident has been very frequent before. The theory justifies this observation. Paper is an imperfect conductor, even when soaked with flour paste; and the transfusion, though rapid, is not instantaneous nor desultory, but begins faintly, and swells to a maximum. It operates on the glass, like gradual warming instead of the sudden application of great heat.

167. Mr. Cuthbertson, an excellent artist in all electrical apparatus, and inventor of the best air-pump, has made a curious observation on this subject. He says that he has uniformly observed, that jars take a much greater charge (nearly one third), if the inside be considerably damped, by blowing into it with a tube reaching to the bottom (*Nicholson's Journal, March* 1799).—We must acknowledge, that we can form no distinct conception of what Mr. Cuthbertson calls *an undulation of the elastic atmosphere.* We do not know whether he means that the atmosphere is actually undulating as water, or as air in the production of sound, its parts being in a reciprocating motion; or whether he only means that this atmosphere consists of quiescent strata, alternately denser and rarer. Nor can we form any notion how either of these undulations contributes to the explosion, or prevents it. We are really but very imperfectly acquainted with that part of the science which should determine the precise accumulation that produces the desultory transference. We mentioned one necessary consequence of the action inversely as the square of the distance, which has some relation to this question, *viz.* that a particle, making part of a spherical surface, is twice as much repelled when it has

just quitted the surface as when it made part of it, provided its place be immediately supplied. And another circumstance has been frequently mentioned, *viz.* that a greater, and perhaps much greater, force is necessary, for enabling a particle of fluid to quit the last series of particles of the solid matter than for producing almost any constipation. But we are not certain that these circumstances are of sufficient influence to explain the whole of the event. *Valeant quantum valere possint.* Yet we are of opinion that Mr. Cuthbertson has assigned the true cause, namely, the imperfect coating of the inside of the glass. When we come to the explanation of the escape of electricity along imperfect conductors, we hope that it will appear, that the disposition to escape must be greatly diminished by a charge, which disposes the fluid so, that in no place the constipation is remarkably greater than in another part very near it, and the density changes everywhere slowly.

168. With respect to the form of the coated glass, the theory prescribes that which will occasion such a distribution of the electric fluid as shall make its repulsion for the fluid in the canal which connects it with the prime conductor as little as possible. In this respect, it would seem that a plate is the best, and a globe the worst: but if both are very thin, the difference cannot be considerable. Our experience, however, seems to indicate the opposite maxim as the most proper. We have uniformly found a globe to be far preferable to a plate of the same thickness, and that a plate was generally the weakest form. It must be owned, that we have not yet been able to ascertain by the theory what is the exact distribution of the redundant fluid in a plate. In a sphere, it must be uniformly spread over the surface. We must also ascribe part of the inferiority of the plate to its greater tendency to dissipation from the edges. If a plate be coated in a star-like form, with slender projecting points, we shall observe them luminous in the dark, almost at the beginning of the accumulation; and the plate will discharge

itself by these points, over the uncoated part, before it has attained any considerable strength. Those forms are least exposed to this deterioration which have the least circumference to the same quantity of surface. We have always found, that a square coating will not receive a more powerful charge without exploding than a circular one of the same breadth, although it contains a fourth more surface; and this although any visible escape from the angles be prevented by covering the outline with sealing wax. Of all forms, therefore, a globe, with a very narrow, but long neck, is the most retentive. But it is very difficult to coat the inside of such a vessel. The balloons used in chemical distillations make excellent jars, and can be easily coated internally when the neck will admit the hand. The thinnest of tinfoil may be used, by first pasting it on paper, and then applying it either with the foil or the paper next the glass. It should be cut into gussets, as in the covering of terrestrial globes; and they should be put on overlapping about half an inch. The middle of the bottom is then coated with a circular piece. The great bottles for holding the mineral acids are also good jars, but inferior to the balloons, because they are very thick in the bottom, and for some distance from it. A box of balloons contains more effective surface than an equal box of jars of the same diameter and height of coating.

169. The most compendious battery may be made in the following manner: Choose some very flat and thin panes of the best crown glass, coat a circle *(a b c d)*, (Plate II. fig. 7.) in the middle of both surfaces, so as to leave a sufficient border uncoated for preventing a spontaneous discharge; let each of them have a narrow slip of tinfoil *a* reaching from the coating to the edge on one side, and a similar slip *c* leading to the opposite edge on the other side. Lay them on each other so that the slips of two adjoining plates may coincide. Connect all the ends of these slips on one side together by a slip of the same foil, or a wire which touches them all. Then, connecting one of these collecting slips with the prime con-

ductor, and the other with the ground, we may charge and discharge the whole together. If the panes be round, or exact squares, we may employ as few of them together as we please, by setting the whole in an open frame, like an old-fashioned plate-warmer; and then turning the set which we would employ together at right angles to the rest. This evidently detaches the two parcels from each other. This battery may be varied in many ways; and if the whole is always to be employed together, we may make it extremely retentive, by covering the uncoated border of the plate with melted pitch, and, while it is soft, pressing down its neighbour on it till the metallic coatings touch. For greater variability this may be done in parcels of the whole.

170. On the same principle, a most compendious battery may be made by alternate layers of tinfoil and hard varnish, or by coating plates of very clear and dry Muscovy glass. But these must be used with caution, lest they be burst by a spontaneous discharge; in which case we cannot discover where the flaw has happened. They make a surprising accumulation, without shewing any vivid electricity.

171. We have made a very fine electric phial for carrying about, by forming tin-plate (iron plate tinned) into somewhat of a phial shape, with a long neck. We then covered this with a coating of fine sealing wax, about $\frac{1}{30}$th of an inch thick, quite to the end of the neck, and coated the sealing wax, all but the neck, with tinfoil. It is plain that the sealing wax is the coated idio-electric, and that the tin-plate phial serves for an inner coating and wire. The dissipation is almost nothing if the neck be very small; and it only requires a little caution to avoid bursting by too high a charge. Even this may be prevented by coating the sealing wax so near to the end of the neck, that a spontaneous discharge must happen before the accumulation is too great.

172. It is well known that the discharge happens when the discharging balls are at a considerable distance from each other; therefore only as much is discharged as corresponds

to that distance. This is one cause of the residuum of a discharge which sometimes is pretty considerable. Some experiments require the very utmost force of the charge. It is therefore proper to make the discharge as close and abrupt as possible. But the most rapid approach that we can make of the discharger is nothing in comparison with the velocity with which the fluid seems to fly off, and will therefore have but small influence in making a more instantaneous and complete discharge. Theory points out the following method: Let a very thick plate of glass (half an inch), of several inches diameter, be put between the discharging balls, which should, in this case, be small, and let these balls be strongly pressed against it by a spring. While the charge is going on, a very small part of the glass plate, round the points of contact, will receive a weak and useless charge; but this will not hinder the battery from acquiring the same intensity of charge. When this is completed, let the intervening glass plate be briskly withdrawn. The discharge will begin with an intensity which is unattainable in the ordinary manner of proceeding.

173. Much has been said of the lateral explosion. It appears, that in some of the prodigious transferences of electricity that have taken place in the discharge of great surfaces through wires barely sufficient to conduct them, flashes of light are thrown off laterally; but the most delicate electrometer, it is said, is not affected. The fact is not accurately narrated; we have always observed a very delicate electrometer to be affected. The passage of such a quantity of fluid is almost equivalent to the co-existence of it in any given section of the wire; but it remains there for so short a time, that, acting as an accelerating force, it cannot produce a very sensible motion. It is like the discharging a pistol ball through a sheet of paper hanging loosely. It goes through it without very sensibly agitating it.

174. It has sometimes appeared to us probable that, by means of this lateral explosion, the direction of the current may

be discovered. Let the jar *a b* (Plate II. fig. 8.) be discharged by a wire *a c d e b*, interrupted at *c d* by the coating of a very thin plate of talc; let the coating also be very thin. There must be *some* obstruction to the motion, which must cause the fluid to press on the sides or surfaces of the coating, just as the obstruction to the motion of water in a pipe (arising from friction, or even from material obstacles in the pipe) causes the water to press on the sides of the pipe. Therefore if a wire *x z* connect the other coating with the ground, we should expect that fluid will be expelled along this wire, and a charge be given to the plate of talc. Now whether the course in this apparatus be from *b* to *a*, or from *a* to *b*, if any charge be acquired by *c d*, it will *probably* be positive in *c d*, and negative in *x z*; for it is electric fluid that is supposed to pass: therefore we should always have one species of electricity, whether *a* has been charged by glass or by sealing wax; and this species will indicate which is positive. We have said "*probably*"—for it is not impossible that it may be otherwise. If the abstraction at *d* be supposed more powerful than the supplying force at *c*, the same obstruction may perhaps keep the plate *c d* in an *absorbing* state, just as water descending in a vertical pipe, into which it is pressed by a very small head of water in the cistern, instead of pressing the sides of the pipe, rather draws them inwards, as is well known. This seems, at any rate, an interesting experiment; for we must acknowledge, that there still hangs a mysterious curtain before a theory which deduces so much from the presence of a substance which we have never been able to exhibit alone, and where we do not know when it abounds and when it is deficient. It is like the phlogiston of Stahl, or the caloric of Lavoisier. It will be proper to use the thinnest plate of talc to be charged, and to connect it with another coated plate of half the diameter, or less, in order to increase the accumulation. It seems by no means a desperate case.

The theory of coated glass now explained, might have been treated with more precision, and the formulæ deduced in the beginning of this article might have been employed for stating the sum total of the acting forces, and thus demonstrating with precision the truth of the general result; and indeed it was with such a view that they were premised: but they would have been considerably complicated in the present case; for however thin we suppose the tinfoil coatings to be, it is evident from § 92, &c. that each coating will consist of three strata; of which the two outermost are active, and must have their forces stated, and the statement of the force of each stratum would have consisted of three terms. This would have been very embarrassing to some readers; and the force of the conclusion would not, after all, have been much more convincing than we hope the above more loose and popular account has been.

175. We have hitherto considered the non-electric coatings only, and have not attended to what may chance to obtain in the substance of the coated electrics themselves. May not part, at least, of the redundant fluid be lodged in one superficial stratum of the glass? or, if it do not penetrate it, may it not adhere to the surface, and drive off from the other surface, or stratum, a part of what naturally adheres to it? Till Dr. Franklin's notions on the subject became prevalent, no person doubted this. The electric was supposed to contain or to accumulate in its surface all the electricity that we know. But the first suggestion of Dr. Franklin's experiments certainly was, that the electric plate or vessel acted merely as an obstacle, preventing the fluid from flying from the body where it was redundant to that where it was deficient. It is therefore an important question in the science, whether the glass or electric concerned in these phenomena serve any other purpose besides the mere prevention of the redundant fluid from flying to the negative plate?

176. Now it appears, at the very first, that this is the case.

For if a glass be coated only on one side, and be electrified on that side, we obtain a strong spark from the other side by bringing the knuckle near it; and this may be obtained for some time from one spot of that surface; and after this we get no more from that spot, but get sparks, with the same vivacity, and in the same number, from any other spot that is opposite to the coating on the other side. In this manner we can obtain a succession of sparks from every inch of surface opposite to the coating, and from no other part. But what puts this question beyond all doubt is, that if we now lay a metal coating on the surface from which the sparks have been drawn in this manner, and make a communication between the two metallic coatings, by means of a bent wire, we obtain a perfect discharge. To complete the proof, we need only observe that this experiment succeeds whether the glass has been electrified by excited glass or by excited sealing-wax. Therefore the coated surface may receive the electric fluid by the coating, as we see plainly that it is abstracted by the coating. The use of the coatings may be nothing more than to act as conductors to every part of the surface of the electric. None of these thoughts escaped the penetrating and sagacious mind of Dr. Franklin. He immediately put it to the test of experiment; and laying a moveable metallic coating on both surfaces, he found the glass charge perfectly well. He lifted off the coatings: which operation was accompanied by flashes of light between the metallic coverings and the glass from which he separated them. Having removed the coatings, he applied others, completed the circle, and obtained a perfect discharge, not distinguishable from what he would have obtained from the first coatings.

177. Thus it was demonstrated, that the glass plate itself acquired by charging a redundant stratum on one side, and a deficient stratum on the other side; and we now see, at once, the reason why the accumulation turns out greater than what is determined by the theory. The distance be-

tween the redundant and deficient stratum is less than the thickness of the glass; and this, perhaps, is an unknown proportion.

This precious experiment of Dr Franklin was repeated by every electrician, and varied in a thousand ways. No philosopher has carried this research farther than Beccaria; and he has given ground for a most important discovery in the mechanical theory, namely, that the charged glass has several strata, of inconceivable thinness, alternately redundant and deficient in electric fluid; and that by continuing the electrification, these strata penetrate deeper into the glass, and probably increase in number. We have not room here to give even an account of his experiments, and must refer the philosophical and curious reader to that part of his valuable Treatise where he treats of what he calls *vindicating or recovering electricity;* as also to a paper by Mr. Henley in Phil. Trans. for 1766, giving an account of experiments on Dutch plates by Mr. Lane. The general form of the experiment is this. He puts two plates together; he coats the outer surfaces, and charges and discharges them as one thick plate. Their inner touching surfaces are found strongly electrical after the discharge, having opposite electricities, and changing these electricities by repeated separations and replacings, in a way seemingly very capricious at first sight, but which the attentive reader will find to be according to fixed laws, and agreeably to the supposition that the strata gradually shift their places within the glass, very much resembling what we observe in a long glass rod which we would render electric by induction. In this case, as was observed in § 57. there are observed more than one neutral point, &c.

178. Mr. Cavendish endeavours to give us some notion of the disposition of the fluid in the substance of the glass in the following manner: Having separated the coated plate from the machine and from the ground, suppose a little of the redundant fluid in B $\beta\,\delta$ D (Plate II. fig. 9.) equal to the fluid wanting in E $\varepsilon\,\phi$ F. If we now suppose all the redund-

ant fluid to be lodged in $b\beta\delta d$, and $e\varepsilon\varphi f$ to hold all the redundant matter, and the two coatings to be in their natural state, a particle p, placed in the middle of the surface $b\,d$, will be nearly as much attracted by $\varepsilon e f \varphi$ as it is repelled by $b\beta\delta d$ (exactly so if the plates were infinitely extended); and if the coating be removed, keeping parallel and opposite to the surface that it quits, there will be very little, if any, tendency to fly from the glass to the coating; there will rather be some disposition in the fluid to quit the coating and fly to the glass; because the repulsion of $b\beta\delta d$ is more diminished than the attraction of $\varepsilon e\varphi f$. (§ 42.) But the difference will be very small indeed. (*N. B.* The result would be very different if electric action followed a different law. Were it as $\frac{1}{d^3}$, the coating would be much overcharged; and were it as $\frac{1}{d}$, it would be very much undercharged.)

Now the fact is, that when the coating is carefully removed, it is possessed of very little electricity, not more than may reasonably be supposed to run into it by bringing away one part before another. It is impossible to keep it mathematically parallel.

Hence we may conclude that the greatest part of the redundant fluid is lodged in the glass if the plates be thin, and the redundant fluid bear but a small proportion to the natural quantity. Similar reasoning shews that the greatest part of the deficiency is in the other side of the glass; and that therefore the coatings are very nearly in their natural state, and merely serve the purpose of conducting.

We have employed coatings of considerable thickness, having holes through them, opposite to which was some gold leaf of the heaviest sort, and almost free of cracks. We have examined the state of the bottom of those pits in Mr. Coulomb's manner, and always found them void of electricity.

179. Thus we learn that glass, and probably all other electrics, acquire redundant and deficient strata as well as the most perfect conductors, at the same time that they may be impervious to the fluid; and we get some mode of conceiving how the rupture happens by a strong charge. This may very probably happen when the strata have formed, in alternate order, so deep in the glass, that a stratum, in which the fluid is crowded close together, may become contiguous to one deprived altogether of fluid. We cannot, however, say with confidence, what *should* be the effect of this state of things; or of one constipated stratum coming in contact with another.

This view of the condition of charged glass explains (we think) several phenomena which seem not well understood by electricians.

180. The residuum of a discharge is frequently owing to a charge extending beyond the coating, where the action is considerably irregular, or different from what it would be if the plates were infinitely extended. This *outline* charge is taken up by the coated part after a very little while, and may again be discharged. But it also frequently arises from another stratum (much thinner, as it will always be) than the exterior one, coming to the surface some time after the first discharge, and being now in a condition for being discharged. It explains the sparkling that is perceived *in succession* between the parts of a jar that is coated in spots, during the charge, and the very sensible residuum of the charge of such a vessel. It explains the phenomena of Beccaria's *Electricitas Vindex* (see ELECTRICITY, *Encycl.*) and the great difference that may be found in the different kinds of glass in this respect. It explains the great difference between the sensation occasioned by a spark from a perfectly conducting surface of considerable extent, and that occasioned by a shock, which conveys the same quantity of fluid accumulated in a small surface of glass. The discharge of the first is almost instantaneous, while that of the last

requires a small moment of time, and is therefore less desultory and abrupt. The one is pungent and startling; but the other is softer in the first instant, and swells to a maximum. Therefore, in the medical employment of electricity, when the purpose is to be effected by the transfusion of a great quantity of electric fluid, we should recommend very small shocks from a very large surface of coated glass, very faintly electrified, in place of strong sparks. Patients of irritable constitutions are frequently alarmed by the quickness and pungency of strong sparks: but if the balls of Lane's shock-measurer be set so close as to give four or five shocks in each turn of a seven inch cylinder, the shocks are not even disagreeable. The balls should be made of fine cupelled silver: in which case, the surface will never be hurt by the greatest discharge; whereas the discharge of four square feet of coated glass will raise such a roughness on the surface of brass as will cause it to sputter, and destroy entirely the regularity of the expenditure of fluid. The same consideration should make us prefer a jar coated internally with amalgam. This cobweb coating gives a greater softness to the shock. Lastly, we see why a powerful and permanent electricity was not produced in the tube filled with melted sealing wax, and treated as mentioned in § 101. The redundancy and deficiency intended to be produced could only be superficial. And because the wax cooled by degrees from the surface to the axis, and the wax is a conductor while liquid, it must have taken a charge at last; and therefore must appear but faintly electrical.

181. This account of the state of charged glass promises us some assistance in our attempts to conceive what passes in the excitation of glass by friction. It appears from Beccaria's experiments, that the redundant fluid is lodged in the same manner in both cases; for by rubbing one side of a glass tumbler, while points were presented to the opposite surface, and were connected with a wire that communicated with the ground, he gave it a powerful charge.

182. It is observed, that when the laminæ of a piece of Muscovy glass are separated, by pulling them asunder without inserting any instrument between them, they are electrical when separated; one being positive, and the other negative. Must we not conclude from this, that when conjoined they were in the state of charged glass? If we take this view of it, a body may contain a prodigious quantity of electric fluid without exhibiting any appearance of it. Mr. Nicholson found, by a very fair computation from his experiments, that a cubic inch of talc, when split into plates of 0,011 of an inch in thickness, and coated with gold leaf, gave a shock equal to the emptying 45 conductors, each seven inches in diameter and three feet long, electrified so that each gave a spark at nine inches distance. Now, the whole of this was moveable fluid, and no more than what the talc contains when unelectrified: for no more comes into the positive side than goes out of the negative side. Nay, there is no probability that the quantity moveable in our experiments bears a considerable proportion to the natural quantity. The quantity of moveable fluid in a man's body is therefore very great: and Lord Mahon is well authorised to say, that the sudden displacing of this quantity in a *returning stroke*, which has been occasioned by a discharge of a cloud in a very distant place, is fully adequate to the production of the most violent effects. But his Lordship has not attended to the circumstance, that no such displacement can happen. The accumulation that can be made in the human body is only superficial; and therefore, although the whole fluid of a man's body may change its place, it will not change it with the rapidity that seems necessary for the violent effects of electricity, except in the very points of communication with the surrounding bodies.

183. We have now seen in what sense the idio-electrics may be said to be impervious to the electric fluid. It is moved in them only to very small and imperceptible distances. When a considerable stratum is discharged, the fluid does

not come from the extremity of it to the point of discharge through the glass, but through the coating. And when alternate strata of redundant fluid and redundant matter are formed, the particles in each shift their places very little, moving perpendicularly to the stratum.

184. Even this degree of obstruction has been denied by some very active electricians, who have multiplied experiments to prove that the fluid passes freely through glass, and that the theory of coated electrics is totally different from what Franklin imagines. Mr. Lyons of Dover has published a numerous list of singular experiments, which he has made with this view, with much trouble, and no small expence. They may all be reduced to this: A wire is brought from the outside of a phial, charged by the knob, and terminates in a sharp point at a small distance from a thin glass plate (it is commonly introduced into a glass tube, having a ball at the end, and the point of the wire reaches to the centre of the ball); and another wire is connected with the discharging rod, and also comes very near (and frequently close) to the other side of the glass, opposite to the pointed wire. With this apparatus he obtains a discharge; and therefore says, that the glass is permeable to electricity. But he does not narrate all the circumstances of the experiment. We have repeated all of them that have any real difference (for most of them are the same fact in different forms), and we have obtained discharges: But they were all very incomplete, except when the glass was perforated, which happened very frequently. The discharge was never made with a full, bright, undivided spark, and loud snap; but with sputtering, and trains of sparks, continued for a very sensible time; and the phial was never deprived of a considerable part of its charge; and (which Mr. Lyons has taken no notice of) the glass is found to be charged, negative on the side connected with the positive side of the phial, and positive on the other. This charge was communicated to the glass over a pretty considerable surface round the

points immediately opposite to the wires. This is quite conformable to the experiments of Dr. Franklin and Beccaria, who charged a tumbler by grasping it with the hand, and presenting the inside to a point electrified by the prime conductor. The whole experiment is analogous to the one narrated in § 176.

185. We may conclude our observations on coated glass with mentioning a curious experiment. A flat stick of fine sealing wax, warmed till it bent pretty readily, was rendered permanently electrical, with a positive and negative pole, in a manner analogous to the double touch of magnets. A small jar was taken, having a hemisphere on the end of its inside wire, and another on the end of a stiff wire projecting from the outer coating, and then turned up parallel to the inside wire; so that the two hemispheres stood equally high, and about three inches asunder. This jar was electrified so weakly, as to run no risk of a spontaneous discharge. The flat faces of the two hemispheres were now applied to the flat side of the sealing wax, and were moved to and fro along it, overpassing both ends about an inch with each hemisphere. The experiment was very troublesome; for the phial often discharged itself along the surface of the sealing wax, and all was to begin again. But, by continuing this operation till the sealing wax grew quite cold and hard, it acquired a very sensible electricism, which lasted several weeks when kept with care; but still it was not much more sensible than that of the sealing wax, which congealed between two globes oppositely electrified.

186. After this application of the theory to the phenomena of coated glass, it will not be necessary to employ much time in its application to the electrophorus. The general propositions from § 14. to 25. and their companions in § 38—43. will enable us to state with precision (when combined with the law of electric action) the actions of every part of this apparatus; and considerable assistance will be derived from a careful consideration of our analysis of Professor

Richmann's experiment in § 156. But we must content ourselves with a general, popular view of these particulars, which may be sufficient for making us understand what will be the *kind*, and somewhat of the *intensity*, of the action of its different parts.

The electrophorus consists of three parts. The chief part is the cake ABCD (Plate II. fig. 10.) of some electric; such as gum lac, sealing wax, pitch, or other resinous composition. This is melted on some conducting plate, DCFE, and allowed to congeal; in which state it is found to be negatively electric. Another conducting plate GHBA is laid on it, and may be raised up by silk lines, or any insulating handle. We shall call ABCD the CAKE, DCFE the SOLE, and GHBA the COVER.

The general appearances not having been so scientifically classed in the article ELECTRICITY as could be wished, we shall here narrate them, very briefly, in a way more suited to our purpose. In comparing the theory with observation, it will be proper to make all the three parts of considerable thickness, and of no great breadth. Although this diminishes greatly the most remarkable of the actions, it leaves them sufficiently vivid, and it greatly increases the smaller changes which are instructive in the comparison. The general facts are,

1. If the sole has been insulated during the congelation of the electric, till all is cold and hard, the whole is found negatively electric, and the finger draws a spark from any part of it, especially from the sole. If allowed to remain in this situation, its electricity grows gradually weaker, and at last disappears: but it may be excited again by rubbing the cake with dry warm flannel, or, which is the best, with dry and warm cat or hare fur. If the cover be now set on the cake by its insulating handle, but without touching the cover, and again separated from the cake, no electricity whatever is observed in the cover.

2. But if it be touched while on the cake, a sharp pungent spark is obtained from it; and if, at the same time, the sole be touched with the thumb, a very sensible shock is felt in the finger and thumb.

3. After this, the electrophorus appears quite inactive, and is said to be *dead;* neither sole nor cover giving any sign of electricity. But,

4. When the cover is raised to some distance from the cake (keeping it parallel therewith), if it be touched while in this situation, a smart spark flies, to some distance, between it and the finger, more remarkably from the upper side, and still more from its edge, which will even throw off sparks into the air, if it be not rounded off. As this diminishes the desired effects, it is proper to have the edge so rounded. This spark is not so sharp as the former, and resembles that from any electrified conductor.

5. The electricity of the cover, while thus raised, is of the opposite kind to that of the cake, or is positive.

6. The electricity of the cover while lying on the cake is the same with that of the cake, or negative.

7. The appearances § 2, 3, 4, may be repeated for a very long time without any sensible diminution of their vivacity. The instrument has been known to retain its power undiminished even for months. This makes it a sort of magazine of electricity, and we can take off the electricity of the cake and of the cover as charges for separate jars, the cover, when raised, charging like the prime conductor of an ordinary electrical machine; and, when set on the cake, charging it like the rubber. This caused the inventor, Mr. Volta, to give it the name of ELECTROPHORUS.

8. If the sole be insulated before putting on the cover, the spark obtained from the cover is not of that cutting kind it was before: but the same shock will be felt if both cake and cover be touched together.

9. If the cover be again raised to a considerable height, the sole will be found electrical, and its electricity is that of the cake and opposite to that of the cover.

10. After touching both cover and sole, if the cover be raised and again set down, without touching it while aloft, the whole is again inactive.

11. If both cover and sole be made inactive when joined, they shew opposite electricities when separated, the sole having the electricity of the cake.

12. If both cover and sole be made inactive when separate, they both shew the opposite to the electricity of the cake when joined.

187. Let us now attend to the disposition of the electrical fluid in the different parts of the instrument in their various situations, and to the forces which operate mutually between them. *N. B.* Experiments for examining this instrument are best made by setting the three plates vertically, supported on glass stalks, with leaden feet to steady them. A very small electrometer may be attached to the outer surfaces of the cover and sole.

If the extent of the plates were incomparably greater than their thickness, we may infer from § 92, &c. that the redundant fluid and matter would be disposed in parallel strata, and that the actions would be the same at all distances. But since this is not the case, the disposition of the fluid will be somewhat different; and whatever it is, the action of any stratum will be diminished by an increase of distance. The following description cannot be very different from the truth:

188.—I. The cake grows negative by cooling; and if it were alone, it would have a negative superficial stratum on both sides, of greater thickness near the edges; and the fluid would probably grow denser by degrees to the middle, where it would have its natural density. This disposition may be inferred from § 92, 93, and 98. But it cools in conjunction with the sole, and the attraction of the redundant matter in the cake for the moveable fluid in the sole disturbs its uniform diffusion in the sole, and causes it to approach the cake. And because this, in all probability, happens while the cake is still a conductor, the disposition of its fluid will

be different from that described above, and the final disposition of the fluid in the cake and sole will resemble that described in § 95, where the plates E and A represent the cake and sole. But because we do not know precisely the gradation of density, and aim only at general notions at present, it will be sufficient to consider the cake and sole as divided into two strata only; one redundant in fluid, and the other deficient, neglecting the neutral stratum that is interposed between them in each. The cake, then, consists of a stratum AB *b a* A containing redundant matter, and a stratum *a b* CD containing redundant fluid: and the sole has a stratum DC *n m* containing redundant fluid, namely, all that belongs naturally to the space DCFE, and a stratum *m n* FE containing redundant matter. This may be called the PRIMITIVE STATE of the cake and sole; and if once changed by communication with unelectrified bodies, it can never be recovered again without some new excitement.

189.—II. If the sole be touched by any body communicating with the ground, fluid will come in, till the repulsion of the redundant fluid in the sole for a superficial particle *y* is equal to the attraction of the redundant matter in the cake for the same particle. What has been said concerning infinitely extended plates, rendered neutral on one side, may suffice to give us a notion of the present disposition of the fluid in the sole. The under surface will be neutral, and the fluid will increase in density toward the surface DC. The sole contains more than its natural quantity of fluid, but is neutral by the balance of opposite forces. Let it now be insulated. This disposition of fluid may be called the *common state* of the electrophorus.

190.—III. Let the cover GHBA be laid on it. The particle *z*, at the upper surface of the cover, must be more attracted by the redundant matter in the stratum A B *b a* than it is repelled by the redundant fluid in the remoter strata; for the fluid in the cake is less than what belongs to it in its natural state, and therefore *z* is attracted by the

cake. The redundant fluid which has come into the remote side of the sole is less than what would saturate the redundant matter of the cake, because it only balances the excess of the remote action of this matter above the nearer action of the compressed fluid in the sole; and this smaller quantity of redundant fluid acts on *z* at a greater distance than that of the redundant matter in the cake. On the whole, therefore, the particle *z*, lying immediately within the surface GH, is attracted; therefore some will move toward the cake, and its natural state of uniform diffusion through the cover will be changed into a violent state, in which it will be compressed on the surface AB, being abstracted from the surface GH. It will now have a stratum G *g p* H, containing redundant matter, and another *g p* BA, containing redundant fluid. But this will disturb the arrangement which had taken place in the sole, and had rendered it neutral on the under surface. We do not attend to the fluid in the cake, but consider it as immoveable, for any motion which it can get will be so small, that the variations of its action will be altogether insignificant. The particle *y*, situated in that surface, will be more repelled by the compressed fluid in the stratum *g p* BA than it is attracted by the equivalent, but more remote redundant matter in GH *p g*. Fluid is therefore disposed to quit the surface EF, and the sole appears positively electric; very little indeed, if the cover be thin. All this may be observed by attaching a small Canton's electrometer to the lower surface of the sole, or by touching the sole with the electrometer of fig. 8, and then trying its electricity by rubbed wax or glass.

191.—IV. A particle of fluid *z*, placed immediately without the surface GH, will be more attracted by the deficient stratum GH *p g* and by AB *b a* than it is repelled by the redundant strata beyond them, and the cover must be sensibly negative. This is the common state of the whole instrument after setting on the cover. It is slightly posi-

tive on the lower surface of the sole, and much more sensibly negative on the upper surface of the cover. A smart spark will therefore be seen between it and the finger, fluid will enter, till the attraction of the redundant matter in AB *b a* is balanced by the repulsion of the redundant fluid in DCFE.

192.—V. A spark will now be obtained from the sole, because it was faintly positive before, and there has been added the action of the fluid which has entered into the cover. The fluid in the sole is therefore disposed to fly to any body presented to it. But when this has happened, the equillibrium at the surface GH is destroyed, and that surface again becomes negative, and will attract fluid, although the cover already contains more than its natural quantity. A small spark will therefore be seen between the cover and any conducting body presented to it. By touching it, the neutrality or equilibrium is restored at GH; but it is destroyed again at EF, which will again give a positive spark, which, in its turn, again leaves GH negative. This will go on for ever, in a series of communications continually diminishing so as soon to become insensible, if the three parts of the electrophorus be thin. This makes it proper to make them otherwise, if the instrument be intended for illustrating the theory.

At last the equilibrium is completed at the surfaces GH and EF, and both are neutral in relation to surrounding bodies, although both the cover and sole contain more than their natural share of electric fluid. We may call this the NEUTRAL or DEAD state of the electrophorus.

193. This state may be produced at once, instead of doing it by these alternate touches of GH and EF. If we touch at once both these surfaces, we have a bright pungent spark, and a small shock. If this be the object of the experiment, the state N° IV. which gives occasion to it, may be called the CHARGED state of the electrophorus.

When the instrument has thus been rendered neutral in relation to surrounding bodies, it is plain that it may con-

tinue in this state for any length of time without any diminution of its capability of producing the other phenomena, provided only that no fluid pass from the cover to the cake. We do not *fully* understand what prevents this communication, nor indeed what prevents the rapid escape from an overcharged body into the air. This cause, whatever it be, operates here; and the best way of preventing the dissipation, or the absorption by the cake, is to keep the electrophorus with its cover on. It will come into this neutral state by dissipation from the sole, and absorption by the cover, in no very long time; and after this, will remain neutral, retaining its power with great obstinacy, especially if the cake and plates are very thin.

194.—VI. If the cover be now removed to a distance, both parts of the apparatus will shew strong marks of electricity. The cover contains much redundant fluid, and must appear strongly positive, and will give a bright spark, which may be employed for any purpose. It may be employed for charging a jar positively by the knob, if we just touch the cover with the knob. The sole will attract fluid, or be negative, although it contain more than its natural quantity of fluid, and it will take a spark. The sole therefore, in the absence of the cover, may be employed to charge a jar negatively by the knob. By touching it with the finger, or with the knob of a jar held in the hand, it is reduced to the common state described in N° II.; and now all the former experiments may be repeated. We may call this the ACTIVE or the CHARGING state.

This state of the apparatus has caused it to get the name *Electrophorus*. Volta, its undoubted inventor, called it *Electroforo perpetuo;* for it *appears*, as has been already observed, to contain a magazine of electricity. The cover, when removed, will charge a jar held in the hand positively; and having done this service, it will charge a jar negatively when again set on the cake. The sole, in the absence of the cover, will charge a third jar negatively: and then, when the

cover, after being touched, is set down again, it will charge a fourth jar positively. It will not be difficult to contrive a simple mechanism, connected with the motion of the cover, which shall connect the joined parts with two jars, and shall connect them, when separated, with two others; and thus charge all the four with great expedition. All this is done without any new excitation of the electrophorus. But it is by no means a *magazine* of electricity which it gradually expends: it is a COLLECTOR of electricity from the surrounding bodies, which it afterwards imparts to others, and may be employed to discharge jars in the same gradual manner as to charge them.

195.—VII. If the electrophorus is not insulated, a shock may still be obtained, by first touching the sole, and then, without removing the finger, touching the cover: but this will not be so smart as when the negative cover is touched at the same time that we touch the sole, more highly positive, than when it communicates with the ground. The difference must, however, be almost imperceptible when the pieces are thin.

196.—VIII. If the electrophorus is not insulated, the cover, when put on, will give a spark in the manner already mentioned, and it will be somewhat stronger than when it is insulated; because the fluid is allowed to escape from the sole, and does not obstruct the entry into the cover. If we then, without removing the finger from the cover, touch the sole, nothing is felt; but if we first touch the sole, and without removing the finger from it, touch the cover, we obtain a shock. This is evident from the theory. By this series of alternate touches, the period of the electrophorus is completed. The electrophorus is charged, or rendered neutral, by touching the plates when joined; then, by touching both when separated, the whole is reduced to the common state. When separated, from being in the neutral state, they have opposite electricities, the sole shewing that of the cake

When brought together, each in the common state, they have opposite electricities, the cover shewing that of the cake.

197.—IX. When, by long exposure to the air without its cover, the electrophorus has lost its virtue, it may be brought again into an active state in a variety of ways. Its surface may be rendered negative by friction with dry cat or hare skin, or warm flannel. It may be rendered negative by setting on it a jar charged positively on the inside, and then touching the knob with any thing communicating with the ground. This is the most expeditious method, and will give it a high degree of excitation, if the jar be of size, and if the electrophorus be covered with a plate of tinfoil which comes into contact all over its surface. This however requires the previous charging of the jar; therefore it will be as expeditious and effectual to connect this surface with the rubber of an electrical machine. We had almost forgotten to remark, that the effects of bringing the cover edgewise to the cake follow clearly from the theory, as will appear to the attentive reader without further explanation.

198. The electrophorus has been compared to a charged plate of coated glass. It is true that it *may be brought* into an external state which very much resembles a charged pane; namely, when the cover, in its natural state, is set on the electrophorus in its natural state; and accordingly it gives a shock, and the two exterior surfaces become neutral; but the internal constitution and the acting forces, are totally and *essentially* different. The two coatings of the pane would not, when separated, exhibit the appearances of the electrophorus; nor, when touched in their disjoined state, will they produce the same effects when joined. In the operation of coated glass, the constant or invariable part, the glass is not the *agent*, it is merely the *occasion* of the action, by *allowing* the accumulation. In the electrophorus, the electric, which is the constant invariable part, is the *agent producing* the accumulation. The electrophorus is an original, and a very ingenious and curious electrical machine. Nothing has so much contributed to spread some general, though slight, ac-

quaintance with the mechanical principles of electricity. The numerous dabblers in natural knowledge had been diverted from scientific pursuit by the variety of the singular and amazing effects of electricity, and had really attained very little connected knowledge. The effects of the electrophorus forced this knowledge on them; because no use can be made of it without a pretty clear conception of the disposition of the electricity, and the kind and intensity of the actions. It is therefore most ungrateful in the experimenters who have attained better views, to attempt to rob Mr. Volta of the real merit of discovery, by shewing that its effects are similar to those of Mr. Symmer's stockings, or of Cigna's plates, or of Franklin's charged or discharged glass panes. And the attempt destroys itself: for it shews the ignorance or inattention of its author; for the similarity is not real, as will appear clear to any person who will examine things minutely and scientifically, proceeding in this examination on suppositions similar to those which we employed in the analysis of Richmann's experiment. It was indeed in subserviency to this examination that we entered into the detail of that experiment, it being a simpler case. The accurate examination of Richmann's experiment requires the fluxionary calculus in its refined form. In the present question five acting strata are to be considered, which renders the formulæ very complicated, and indeed intractable, unless we make the plates extremely thin; which, fortunately, is the best form of the instrument. We have completed this mathematical analysis; and the popular view here given is the result of that computation.

199. The electricians are no less obliged to Mr. Volta for another machine, or instrument, from which the study of Nature's operations has derived, or may derive, immense advantages. We mean the CONDENSER or COLLECTOR of electricity*. The general effect is to render sensible an ac-

* See the Edinburgh Encyclopedia, vol. VIII. p. 525, for an account of this instrument.

cumulation or deficiency of electric fluid so slight that it will not affect the most delicate electrometer; and it produces (at least in the opinion of Mr. Volta) this effect, by employing for the sole of an electrophorus a body which is an imperfect conductor, such as a plate of well dried marble, or well dried, but not baked, wood; or even a conducting body, covered with a bit of dry taffety or other silk. Mr. Volta, Cavallo, and others, who have written a great deal on the subject, have attempted to shew how these substances are preferable (and they certainly are preferable in a high degree) to more perfect insulators: but not having taken pains to form precise notions of the disposition and action of the electric fluid in the situations afforded by the instrument, their reasonings have not been very clear. We think that an adequate conception of the essentials of the proposed instrument may be acquired by means of the following considerations:

200. Furnish the cover of an electrophorus with a graduated electrometer, which indicates the proportional *degrees of electricity*; electrify it positively to any degree, suppose six, while held in the hand, at some distance, right over a metal plate lying on a wine glass as an insulating stand, but communicating with the ground by a wire. Bring it gradually down toward the plate. Theory teaches, and we know it by experiment, that the electrometer will gradually subside, and perhaps will reach to 2° before the electricity is communicated in a spark. Stop it before this happens. In this state the attraction of the lying plate produces a compensation of four degrees of the mutual repulsion of the parts of the cover, by constipating the fluid on its under surface, and forming a deficient stratum above. This needs no farther explanation after what has been said on the charging of coated glass plates. Now we can suppose that the escape of the fluid from this body into the air begins as soon as electrified to the degree 6, and that it will fly to the lying plate with the degree 2, if brought nearer. If we can pre-

and still conducts internally. It is then in the best possible state. The same may be said of dry unbaked wood. Varnishing the upper surface of a piece of marble or wood is equivalent to laying a thin glass plate on it. Now this method, or covering the top of the marble, or of a book, or even the table, with a piece of clean dry silk, makes them all the most perfect condensators. This just view of the matter has great advantages. It takes away the mysterious indistinctness and obscurity which kept the instrument a quackish tool, incapable of improvement. We can now make one incomparably better and more simple than any proposed by the very ingenious inventor. We need only the simple moveable plate. Let this be varnished on the under side with a moderately thick coat of the purest and hardest *vernis de Martin*, or coach-painters varnish; and we have a complete condensator by laying this on a table. If it be connected by a wire with the substance in which the weak and imperceptible electricity is excited, it will be raised (provided there be enough of it of that small intensity) in the proportion of the thickness of the varnish to the fourth part of the diameter of the plate. This degree of condensation will be procured by detaching the connecting wire from the insulating handle of the condenser, and then raising the condenser from the table. It will then give sparks, though the original electricity could not sensibly affect a flaxen fibre.

It must be particularly noted, that it can produce this condensation only when there is fluid to condense; that is, only when the weak electricity is diffused over a greater space than the plate of the condenser. In this way it is a most excellent collector of the weak atmospheric electricity, and of all diffused electricity. But to derive the same advantage from it *in many very interesting cases*, such as the inquiry into the electricity excited in many operations of Nature on small quantities of matter, we must have condensers of various sizes, some not larger than a silver penny. To construct these in perfection, we must use the purest and

hardest varnish, of a kind not apt to crack, and highly coercive. This requires experiment to discover it. Spirit varnishes are the most coercive; but by their difference of contraction by cold from that of metals, they soon appear frosty, and when viewed through a lens, they appear all shivered: They are then useless. Oil varnishes have the requisite toughness, but are much inferior in coercion. We have found amber varnish inferior to copal varnish in this respect, contrary to our expectation. On the whole, we should prefer the finest coach-painters varnish, new from the shop, into which a pencil has never been dipped: and we must be particularly careful to clear our pencils of moisture and all conducting matter, which never fails to taint the varnish. We scarcely need remark, that the coat of varnish on these small condensers should be very thin, otherwise we lose all the advantage of their smallness.

201. Mr. Cavallo has ingeniously improved Volta's condenser by connecting the moveable plate, after removal, with a smaller condenser. The effect of this is evident from § 130. But the same thing would have been generally obtained by using the small condenser at first, or by using a still thinner coat of varnish.

202. It will readily occur to the reader, that this instrument is not instantaneous in its operation, and that the application must be continued for some time, in order to collect the minute electricity which may be excited in the operations of nature. He will also be careful that the experiment be so conducted that no useless accumulation is made anywhere else. When we expect electricity from any chemical mixture, it never should be made in a glass vessel, for this will take a charge, and thus may absorb the whole excited electricity, accumulating it in a neutral or insensible state. Let the mixture be made in vessels of a conducting substance, insulated with as little contact as possible with the insulating support; for here will also be

something like a charge. Suspend it by silk threads, or let it rest on the tops of three glass rods, &c.

203. After this account of the Leyden phial, electrophorus, and condenser, it is surely unnecessary to employ any time in explaining Mr. Bennet's most ingenious and useful instrument called the *doubler of electricity*. The explanation offers itself spontaneously to any person who understands what has been said already. Mr. Cavallo has with industry searched out all its imperfections, and has done something to remove them, by several very ingenious constructions, minutely described in his Treatise on Electricity. Mr. Bennet's original instrument may be freed, we imagine, as far as seems possible, by using a plate of air as the intermedium between the three plates of the doubler. Stick on one of the plates three very small spherules made from a capillary tube of glass, or from a thread of sealing wax. The other plate being laid on them, rests on mere points, and can scarcely receive any friction which will disturb the experiment. Mr. Nicholson's beautiful mechanism for expediting the multiplication has the inconveniency of bringing the plates towards each other edgewise, which will bring on a spark or communication sooner than may be desired: but this is no inconvenience whatever in any philosophical research; because, before this happens, the electricity has become very distinguishable as to its kind, and the degree of multiplication is little more than an amusement. The spark may even serve to give an indication of the original intensity, by means of the number of turns necessary for producing it. If the fine wires, which form the alternate connections in so ingenious a manner, could be tipped with little balls to prevent the dissipation, it would be a great improvement indeed. An alternate motion, like that of a pump-handle, might be adopted with advantage. This would allow the plates to approach each other face to face, and admit a greater multiplication, if thought necessary

204. One of the most remarkable facts in electricity is the rapid dissipation by sharp points, and the impossibility of making any considerable accumulation in a body which has any such, projecting beyond other parts of its surface. The dissipation is attended with many remarkable circumstances, which have greatly the appearance of the actual escape of some material substance. A stream of wind blows from such a point, and quickly electrifies the air of a room to such a degree, that an electrometer in the farthest corner of the room is affected by it. This dissipation in a dark place is, in many instances, accompanied by a bright train of light diverging from the point like a firework. Dr. Franklin therefore was very anxious to reconcile this appearance with his theory of plus and minus electricity, but does not express himself well satisfied with any explanation which had occurred to him. From the beginning, he saw that he could not consider the stream of wind as a proof of the escape of the electric fluid, because the same stream is observed to issue from a sharp negative point; which, according to his theory, is not dispersing, but absorbing it. Mr. Cavendish has, in our opinion, given the first satisfactory account of this phenomena.

205. To see this in its full force, the phenomenon itself must be carefully observed. The stream of wind is plainly produced by the escape of something from the point itself, which hurries the air along with it; and this draws along with it a great deal of the surrounding air, especially from behind, in the same manner as the very slender thread of air from a blow-pipe hurries along with it the surrounding air and flame from a considerable surface on all sides. It is in this manner that it gathers the whole of a large flame into one mass, and, at last into a very point. If the smoke of a little rosin thrown on a bit of live coal be made to rise quietly round a point projecting from an electrified body, continually supplied from an electrical machine, the vortices of this smoke may be observed to curl in from all

sides, along the wire, forming a current of which the wire is the axis, and it goes off completely by the point. But if the wire be made to pass through a cork fixed in the bottom of a wide glass tube, and if its point project not beyond the mouth of the tube, the afflux of the air from behind is prevented, and we have no stream; but if the cork be removed, and the wire still occupy the axis of the tube, but without touching the sides, we have the stream very distinctly; and smoke which rises round the far end of the tube is drawn into it, and goes off at the point of the wire. Now it is of importance to observe, that whatever prevents the formation of this stream of wind prevents the dissipation of electricity (for we shall not say escape of electric fluid) from the point. If the point project a quarter of an inch beyond the tube, or if the tube be open behind, the stream is strong, and the dissipation so rapid, that even a very good machine is not able to raise a Henley's electrometer, standing on the conductor, a very few degrees. If the tube be slipped forward, so that the point is just even with its mouth, the dissipation of electricity is next to nothing, and does not exceed what might be produced by such air as can be collected by a superficial point. If the tube be made to advance half an inch beyond the point which it surrounds, the dissipation becomes insensible. All these facts put it beyond a doubt that the air is the cause, or, at least, the occasion of the dissipation, and carries the electricity off with it, in this manner rendering electrical the whole air of a room. The problem is reduced to explain how the air contiguous to a sharp electrified point is electrified and thrown off.

It was demonstrated in § 130, that two spheres, connected by an infinitely extended, but slender conducting canal, are in electrical equilibrium, if their surfaces contain fluid in the proportion of their diameters. In this case, the superficial density of the fluid and its tendency to escape are inversely as the diameters (§ 130) Now if, in imagination, we gradually diminish the diameter of one of the spheres

the tendency to escape will increase in a greater proportion than any that we can name. We know, that when the prime conductor of a powerful table-machine has a wire of a few inches in length projecting from its end, and terminating in a ball of half an inch in diameter, we cannot electrify it beyond a certain degree; for when arrived at this degree, the electricity flies off in successive bursts from this ball. Being much more overcharged than any other part of the body, the air surrounding the ball becomes more overcharged by communication, and is repelled, and its place supplied by other air, not so much overcharged, which surrounded the other parts of the body, and is pressed forwards into this space by the general repulsion of the conductor and the confining pressure of the atmosphere; otherwise, being also overcharged, it would have no tendency to come to this place. Half a turn of the cylinder is sufficient to accumulate to a degree sufficient for producing one of these explosions, and we have two of them for every turn of the cylinder. A point may be compared to an incomparably smaller ball. The constipation of the fluid, and its tendency to escape, must be greater in the same unmeasurable proportion. This density and mutual repulsion cannot be diminished, and must even be increased, by the matter of the wire forming a cone, of which the point is the apex; therefore, if there were no other cause, we must see that it is almost impossible to confine a collection of particles, mutually repelling, and constipated, as these are in a fine point.

206. But the chief cause seems to be a certain chemical union which takes place between the electric fluid and a corresponding ingredient of the air. In this state of constipation, almost completely surrounded by the air, the little mass of fluid must attract and be attracted with very great force, and more readily overcome the force which keeps the electrified fluid attached to the last series of particles of the wire. It unites with the air, rendering it electric in the

highest degree of redundancy. It is therefore strongly repelled by the mass of constipated fluid which succeeds it within the point. Thus is the electrified air continually thrown off, in a state of electrification, that must rapidly diminish the electricity of the conductor. Hence the uninterrupted flow, without noise or much light, when the point is made very fine. When the point is blunt, a little accumulation is necessary before it attains the degree necessary for even this minute explosion ; but this is soon done, and these little explosions succeed each other rapidly, accompanied by a sputtering noise, and trains of bright sparks. The noise is undoubtedly owing to the atoms of the highly electrified fluid. These are, in all probability, rarefied of a sudden, in the act of electrification, and immediately collapse again in the act of chemical union, which causes a sonorous agitation of the air. This electrified air is thus thrown off, and its place is immediately supplied by air from behind, not yet electrified, and therefore strongly drawn forward to the point, from which it is thrown off in its turn. This rapid expansion and subsequent collapsing of the air is verified by the experiments of Mr. Kinnersly, related by Dr. Franklin, and is seen in numberless experiments made with other views in later times, and not attended to. Perhaps it is produced by the great heat which accompanies, or is generated in the transference of electricity, and it is of the same kind with what occasions the bursting of stones, splitting of trees, exploding of metals, &c. by electricity. The expansion is either inconsiderable, or it is successively produced in very small portions of the substance expanded; for when metal is exploded in close vessels, or under water, there is but a minute portion of gazeous matter produced; and in the dissipation by a very fine point, sufficiently great to give full employment to a powerful machine, the stream of wind is but very faint, and nine-tenths of this has been dragged along by the really electrified thread of wind in the middle.

From a collation of all the appearances of electricity, we must form the same conception of the forces which operate round a point that is negatively electrified, not dispersing, but drawing in electric fluid. It is more completely undercharged than any other part of a body, and attracts the fluid in the surrounding air, and the air in which it is retained, with incomparably greater force. It therefore deprives the contiguous air of its fluid, and then repels it, and then produces a stream like the overcharged point.

207. If a conducting body be brought near to any part of an overcharged body, the fronting part of the first is rendered undercharged; and this increases the charge of the opposite part of the overcharged body. It becomes more overcharged in that part, and sooner attains that degree of constipation that enables the fluid to quit the superficial series of particles, and to electrify strongly the contiguous air. The explosion is therefore made in this part in preference to any other; and the air thus exploded is strongly attracted by the fronting part of the other body, and must fly thither in preference to any other point. If, moreover, the fronting part of A be prominent or pointed, this effect will be produced in a superior degree; and the current of electrified air, which will begin very early, will increase this disposition to transference in this way by rarefying the air; a change which the whole course of electric phenomena shews to be highly favourable to this transference, although we cannot perhaps form any very adequate notion how it contributes to this effect. This seems to be the reason why a great explosion and snap, with a copious transference of electricity, is generally preceded by a hissing noise like the rushing of wind, which swells to a maximum in the loud snap itself.

208. If two prominences, precisely similar, and electrified in the contrary way to the same degree, are presented to each other, we cannot say from which the current should take its commencement, or whether it should not equally

begin from both, and a general dispersion of air laterally be the effect; but such a situation is barely possible, and must be infinitely rare. The current will begin from the side which has some superiority of propelling force. We are disposed to think that this current of material electrified substance must suffer great change during its passage, by mixing with the current in an opposite electrical state coming from the other body. Any little mass of the one current must strongly attract a contiguous mass of the other, and certain changes should surely arise from this mixture. These may, in their turn, make a great change in the *mechanical* motions of the air; and, instead of producing a *quaqua versum* dispersion of air from between the bodies, as should result from the meeting of opposite streams, it may even produce a collapsing of the air by the mutual strong attractions of the little masses. Many valuable experiments offer themselves to the curious inquirer. Two little balls may be thus presented to each other, and a smoke may be made with rosin to occupy the interval between them. Motions may be observed which have certain analogies that would afford useful information to the mechanical inquirer. There must be something of this mixture of currents in all such transferences, and the most minute differences in the condition of a little parcel of the air may greatly affect the future motions. The most promising form of such experiment would be to use two points of the same substance, shape, and size, and electrified to the same degree in opposite senses.

209. After all care has been taken to insure similarity, there remains one essential difference, that *the one current is redundant in electric fluid, and the other deficient.* This circumstance *must* produce characteristic differences of appearance. And are there not such differences? Is not the pencil and the star of light a characteristic difference? and does not this well supported fact greatly corroborate the opinion of Dr. Franklin, that the electric phenomena result from

the redundancy and deficiency of *one* substance, and not from two distinct substances operating in a similar manner? For the distinction in appearance is a mechanical distinction. Motion, direction, velocity, are perceivable in it, Locomotive forces are concerned in it; but they are so implicated with forces which probably resemble chemical affinities, hardly operating beyond contact, that to extricate their effects from the complicated phenomenon seems a desperate problem. There is some hitherto inexplicable chemical composition and decomposition taking place in the transference of electricity. Of this a numerous train of observations made since the dawn of the pneumatic chemistry leaves us no room to doubt. The emersion or production of light and heat is a remarkabe sign and proof. Now *this takes place along the whole path of transference;* therefore the process is by no means completed at the point from which the active cause proceeds; and although there be certain appearances that are pretty regular, they are still mixed with others of the most capricious anomaly. The zigzag form of the most condensed spark, totally unlike, by its sharp angles, to any motions producible by accelerating forces, which motions are, without exception, curvilineal, makes us doubt exceedingly whether the luminous lines which we observe are successive appearances of the same matter in different places, or whether they be not rather simultaneous, or nearly simultaneous, coruscations of different parcels of matter in different places, indicating chemical compositions taking place almost at once; and this becomes more probable, when we reflect on what has been said already of the jumbling of opposite currents; such mixtures should be expected. We have seen a darted flash of lightning which reached (in a direction nearly parallel to the horizon) above three miles from right to left; and it seemed to us *to be coexistent;* we could not say at which end it began. The thunder began with a loud crack, and continued with a most irregular rumbling noise about 15 seconds, and seem-

ed equal on both hands. We imagine that it was really a simultaneous snap, in the whole extent of the spark, but of different strength in different places; different portions of the sonorous agitation were propagated to the ear in succession by the sonorous undulationsof air, causing it to seem a lengthened sound. Such would be the appearance to a person standing at one end of a long line of soldiers who discharge their firelocks at one instant. It will seem a running fire, of different strength in different parts of the line, if the muskets have been unequally loaded. It is inconceivable that this long zigzag spark can mark the track of an individual mass of electrified air. The velocity and momentum would be enormous, and would sweep off every thing in its way, and its path could not be angular. The same must be asserted of the streams of light in our experiments. The velocity is so unmeasurable that we cannot tell its direction. There may be very little local motion, just as in the propagation of sound, or of a wave on the surface of water. That particular change of mutual situation among the adjoining atoms which occasions chemical solution or precipitation may be produced in an instant, over a great extent, as we know that a parcel of iron filings, lying at random on the surface of quicksilver, will, in one instant, be arranged in a certain manner by the mere neighbourhood of a magnet. Is not this like the simultaneous precipitation of water along the whole path of a discharge?

But still there must be some cause which gives these simultaneous coruscations a situation with respect to each other, that has a certain regularity. Now the luminous *trains* (for they are not uniform *lines* of light) of almost continuous sparks which are arranged between a positive and a negative point, seem to us to indicate emanation from the positive, and reception by the negative point. The general line has a considerable resemblance to the path of a body projected from the positive point, repelled by it, and attracted by the negative point. This will appear to the mecha-

nician on a very little reflection. If the curve were completely visible, it would somewhat resemble those drawn between P and N in Plate II. fig. 11. PABN overpasses the point N, and comes to it from behind; P *a b* N lies within the other, and arrives in a direction nearly perpendicular to the axis; P α β N describes a straight line, and arrives in the direction PN. As the chemical composition advances, the light is disengaged or produced, and therefore the appearances are more rare as we advance farther in the direction in which they are produced; and there would perhaps be no appearance at all at the point where the motion ends, were it not that the few remaining parcels, where the compositions or decompositions have not been completed, are crowded together at the negative point, *incomparably* more than in any other part of the track. We think that these considerations offer some explanation of the appearance of the pencil and star, which are so uniformly characteristic of the positive and negative electricities; but we see many grounds of uncertainty and doubt, and offer it with due diffidence.

210. The curious figures observed by Mr. Lichtenberg, formed by the dust which settles on a line drawn on the face of a mirror, by the positive and by the negative knobs of a charged jar, are also uniformly characteristic of the two electricities. These are mechanical distinctions, indicating certain differences of accelerating forces. We must refer the curious reader to Lichtenberg's Dissertations in the *Gottingen Commentaries;* to the *Publication of the Haerlem Society;* to the *Gotha Magazine;* to *Dissertations by Spath* at Altdorff, and other German writers*.

211. It only remains for us to take notice of the general laws of the dissipation of electricity into the air, and along imperfect insulators. On this subject we have some valuable

* An account of Lichtenberg's and of Bennet's experiments on these configurations, will be found in the EDINBURGH ENCYCLOPÆDIA, vol. VIII. p. 496, 7.

experiments of Mr. Coulomb, published in the Memoirs of the Academy of Sciences of Paris for 1785.

The general result of Mr. Coulomb's experiments was, that the momentary dissipation of moderate degrees of electricity is proportional to the degree of electricity at the moment. He found that the dissipation is not sensibly affected by the state of the barometer or thermometer; nor is there any sensible difference in bodies of different sizes or different substances, or even different figures, provided that the electricity is very weak.

212. But he found the dissipation greatly affected by the different states of humidity of the air. Saussure's hygrometer has its scale distinctly related to the quantity of water dissolved in a cubic foot of the air. The following little table shews an evident relation to this in the dissipation of electricity:

Hygrometer.	Grains water in cubic foot.	Dissipation per minute.
69	6,197	$\frac{1}{60}$.
75	7,295	$\frac{1}{41}$.
80	8,045	$\frac{1}{29}$.
87	9,221	$\frac{1}{14}$.

Hence it follows, that the dissipation is very nearly in the triplicate ratio of the moisture of the air. Thus if

$\frac{60}{41}$ be considered as $= \overline{\frac{7,197}{6,180}}^{m}$ we have $m = 2,76$.

$\frac{60}{29}$ - - $= \overline{\frac{8,045}{6,180}}^{m}$ gives $m = 2,76$.

$\frac{60}{14}$ - - $= \overline{\frac{9,221}{6,180}}^{m}$ gives $m = 3,61$.

Hence, at a medium, $m = 3,04$.

We should have observed, that the ingenious author took care to separate this dissipation by immediate contact with the air, from what was occasioned by the imperfect insulation afforded by the supports.

213. It must also be remarked here, that the immediate object of observation in the experiments is the diminution of repulsion. This is found to be, in any given state of the air, a certain proportion of the whole repulsion at the moment of diminution: but this is double of the proportion of the density of the electric fluid; for it must be recollected, that the repulsions by which we judge of the dissipation are mutual, exerted by every particle of fluid in the ball t of Coulomb's electrometer, on every particle in the ball a. It is therefore proportional to the electric density of each; and therefore, during the whole dissipation, the densities retain their primitive proportion; therefore, the diminution of the repulsion being as the diminution of the products of the densities, it is as the diminution of the squares of either. If therefore the density be represented by d, the mutual repulsion is representable by d^2, and its momentary diminution by the fluxion of d^2; that is by $2\,d\,\dot{d}$, or $2\,\dot{d} \times d$. Now $2\,\dot{d} \times d$ is to d^2 as $2\,\dot{d}$ is to d; and therefore the diminution of repulsion observed in our experiment bears to the whole repulsion twice as great a proportion as the diminution of density, or the quantity of fluid dissipated, bears to the whole quantity at the moment. For example, if we observe the repulsion diminished $\frac{1}{40}$, we conclude that $\frac{1}{80}$ of the fluid has escaped.

214. Mr. Coulomb has not examined the proportion between the dissipations from bodies of different sizes. A great and a small sphere, communicating by a very long canal, have superficial densities, and tendencies to escape, inversely proportional to the diameters. A body of twice the diameter has four times the surface; and though the tendency to escape be twice as small, the surface is four times as great. Perhaps the greater surface may compensate for the smaller density, and the quantity of fluid actually gone off may be greater in a large sphere. This may be made the subject of trial.

215. It must be kept in mind, that the law of dissipation ascertained by these experiments, relates to one given state of the air, and that it does not follow that in another state, containing perhaps the same quantity of water, the dissipation shall be the same. The air is such a heterogeneous and variable compound that it may have very different affinities with the electric fluid. Mr. Coulomb thought that he should infer from his numerous experiments, that the dissipation did not increase in the ratio of the cube of the water dissolved in the air, unless it was nearly as much as it could dissolve in that temperature. This indeed is conformable to general observation: for air is thought dry when it dries quickly any thing exposed to it; that is, when not nearly saturated with moisture. Now it is well known, that what is thought dry air is favourable to electricity*.

216. The dissipation along imperfect insulators is brought about in a way somewhat different from the manner of its escaping by electrifying the contiguous air and going off with it. It seems to be chiefly, if not solely, along the surface of the insulating support that the electricity is diffused, and that the diffusion is produced there chiefly by the moisture which adheres to it. It is not very easy to form a clear notion of the manner, but Mr. Coulomb's explanation seems as satisfactory as any we have seen.

Water adheres to all bodies, sticking to their surfaces. This adhesion prevents it from going off when electrified; and it is therefore susceptible of a higher degree of electrification. If we suppose that the particles of moisture are uniformly disposed along the surface, leaving spaces between them, the electricity communicated to one particle must attain a certain density before it can fly across the insulating interval to the next. Therefore, when such an imperfect conductor is electrified at one end, the electricity, in passing

* The fullest account of these valuable experiments of Coulomb that has been given in our language, will be found in the EDINBURGH ENCYCLOPÆDIA, vol. VIII. p. 444—449.

to the other, will be weakened at every step. If we take three adjacent particles *a*, *b*, *c*, of this conducting matter, we learn, from § 105, that the motion of *b* is sensibly affected only by the difference of *a* and *c*; and therefore that the passage of electricity from *b* to *c* requires that this difference be superior or equal to the force necessary for clearing this coercive interval. Let a particle pass over. The electric density of the particle *b* of conducting matter is diminished, while the density of the particle on the other side of *a* remains as before. Therefore some will pass from *a* to *b*, and from the particle preceding *a* to *a*, and so on, till we come to the electrified end of this imperfect insulator. It is plain from this consideration, that we must arrive at last at a particle beyond *c*, where the whole repulsion of the preceding particle is just sufficient to clear this interval. Some will come over, whose repulsion, now acting in the opposite direction, will hinder any fluid from supplying its place in the particle which it has quitted. Here the transference will stop, and beyond this the insulation is complete. There is therefore a mathematical relation between the insulating power and the length of the canal, which may be ascertained by our theory; and thus another opportunity obtained for comparing it with observation. That this investigation may be as simple as possible, we may take a very probable case, namely, where the insulating, or, to name it more graphically, the coercive, interval is equal in every part of the canal.

217. Let R be the coercive power of the insulator; that is, let R be the force necessary for clearing the coercive interval. Let a ball C (Plate II. fig. 12.) be suspended by a silk thread AB, and let C represent the quantity of its redundant fluid; and let the density in the different points of the canal be as the ordinates AD, P *d*, &c. of some curve line D *d* B, which cuts the axis in B where the thread begins to insulate completely. Let P *p* be an element of the axis. Draw the ordinate *p f*, the tangent *d f* F, and the nor-

mal d E, and fe perpendicular to P d. Let AC be $= r$, AP $= x$, P $d = y$. Then P $p = \dot{x}$, and $de = -\dot{y}$. We have seen, that the only sensible action on the particle of fluid in P is $-\frac{y\dot{y}}{\dot{x}}$ (see § 105), when the action of the redundant fluid in the globe on the particle P having the density y, is represented by $\frac{C\,y}{(r+x)^2}$. Therefore we have $\frac{y\dot{y}}{\dot{x}} = R$, the coercive power of the thread. This is supposed to be constant. Therefore $\frac{P\,d \times de}{P\,p}$ is equal to some constant line R. But P p, or fe : $de =$ P d : PE. Therefore the subnormal PE is a constant line. But this is the property of the parabola alone; and the curve of density D d B is a parabola, of which the parameter is 2 PE, or 2 R.

218.—*Cor.* 1. The densities in different points of an imperfect insulator are as the square roots of their distance from the point of complete insulation: For P d^2 : AD^2 = BP : BA.

219.—2. The length of canal required for insulating different densities of electricity are as the squares of the densities. For AB $= \frac{AD^2}{2PE}$; and PE has been shown to be a constant quantity. Indeed we see in the demonstration, that BP would insulate a ball, whose electric density is P d, and BA : BP AD^2 : P d^2.

220.—3. The length necessary for insulation is inversely as the coercive force of the canal, and may be represented generally by $\frac{D^2}{R}$. For AB is $= \frac{DA^2}{2PE} = \frac{D^2}{2R}$.

Mr. Coulomb has verified these conclusions by a very satisfactory series of experiments, by the assistance of his delicate electrometer, which is admirably suited for this trial. The subject is so interesting to every zealous student of elec-

tricity, that Mr. Canton, Dr. B. Wilson, Mr. Waitz, Wilcke, and others, have made experiments for establishing some measure of the conducting powers of different substances. It was one of the first things that made the writer of this article suppose that electric action was in the inverse duplicate ratio of the distances: for, as early as 1763, he had found, that the lengths of capillary tubes necessary for insulation were as the squares of the repulsions of the ball which they insulated. The mode of reasoning offers of itself, and the fluxionary expression of the insulating power, *viz.* $\frac{\dot{d}\,\dot{d}}{x}$ led immediately to a force proportional to $\frac{1}{x^2}$. Numerous experiments were made, which we do not give here, because the public are already possessed of those of Mr. Coulomb.

This discussion explains, in a satisfactory manner, the operation of the condenser, as described by Mr. Volta. The weak degrees of electricity, which are rendered sufficiently sensible by the insulation of the plate of dry marble, are completely insulated by the perhaps thin stratum that has been sufficiently dried, while the rest conducts with an efficacy sufficient for permitting the accumulation.

221. When we reflect on the theory now delivered, we see that the formulae determine the distribution of the fluid along an imperfect conductor in a certain manner, on the supposition that a *certain determinate* dose has been imparted to the ball: Because this dose, by diffusing itself from particle to particle of the conducting matter, will diffuse itself all the way to B, in such a manner that the repulsion shall everywhere be in equilibrio with the *maximum* of the coercive force of the insulating interval. But it must be farther noticed, that this resistance is not *active*, but coercitive, and we may compare it to friction or viscidity. Any repulsion of electric fluid, which falls short of this, will not disturb the stability of the fluid spread along the canal, according to any law whatever. So

that if AD represent the electric density of the globe, and remain constant, any curve of density will answer, if $\frac{d\dot{d}}{\dot{x}}$ be everywhere less than R. It is therefore an indeterminate problem to assign, in general, the disposition of fluid in the canal. The density is as the ordinates of a parabola only on the supposition that the maximum of R is everywhere the same. And, in this case, the distance AB is a minimum: for, in other cases of density, we must have $\frac{d\dot{d}}{\dot{x}}$ less than R. If, therefore, we vary a single element of the curve D *d* B, in order that the stability of the fluid may not be disturbed, having $\dot{d}$ constant, we must necessarily have $\dot{x}$ larger, that $\frac{d\dot{d}}{\dot{x}}$ may still be less than R; that is, we must lengthen the axis.

We see also, that to ascertain the distribution in a conducting canal is a determinate problem; whereas, in imperfect conductors, it is indeterminate, but limited by the state of the fluid, when it is so disposed that in every point the action of the fluid is in equilibrio with the maximum of resistance. This consideration will be applied to a valuable purpose in the article MAGNETISM.

222. This doctrine gives, in our opinion, a very satisfactory explanation of the curious observations of Mr. Brookes and Mr. Cuthbertson, mentioned in § 167, namely, that damping the inside of a coated jar diminishes the risk of explosion, and enables it to hold a higher charge. We learn here, that there is no density so great but that the least imperfect conductor will insulate it, if long enough; and that the coercive quality of an imperfect conductor may be conceived so constituted from A towards B, that the densities shall diminish in any ratio that we please, so that the variation of density (the cause of motion) may everywhere, even to the insulating point B, be very small. However great the constipation at the edge of the metallic coating may be, an imperfect con-

ductor may be continued outward from that edge, and may be soconstituted, that the constipation shall diminish by such gentle gradations, that an explosion shall be impossible. An uniform dampness will not do this, but it will diminish the abruptness of the variation of density. The state of density beyond the edge of the coating of a charged jar, very clean and dry, may be represented by the parabolic arch D *i a*. This may be changed by damping, or properly dirtying (to use Mr. Brookes' phrase), to D *f* B; which is evidently preferable. We think it by no means difficult to contrive such a continuation of imperfectly conducting coating. Thus, if gold leaf can be ground to an impalpable powder, it may be mixed with an oil varnish in various proportions. Zones of this gold varnish may be drawn parallel to the edge of the coating, decreasing in metal as they recede from the edge. By such contrivances it may be possible to increase the retentive power to a great degree.

223. This doctrine farther teaches us, that many precautions must be taken when we are making experiments from which measures are to be deduced; and it points them out to the mathematician. In particular, when bodies, supported by insulators, are electrified to a high degree, the supports may receive a quantity of fluid, which may greatly disturb the results; and this quantity, by exerting but a weak action on the parts of the canal, may continue for a very long time, and not be removed but with great difficulty. In such cases, it will be necessary to use new supports in every experiment. From not knowing, or not attending to this circumstance, many erroneous opinions have been formed in some delicate departments of electrical research.

Mr. Coulomb's experiments on this subject are chiefly valuable for having stated the relation between the intensity of the electricity, or, as he expresses it, the electric density, and the lengths of support necessary for the complete insulation. But, as the absolute intensities have all been measured by his electrometer, and he has not given its particu-

lar scale, we cannot make much use of them till this be done by some electrician.

224. Mr. Coulomb found, that a thread of gum lac was the most perfect of all insulators, and is not less than ten times better than a silk thread as dry as it can be made, if we measure its excellence by its shortness. In a considerable number of experiments, he found that a thread of gum lac, of 1,5 inches long, insulated as well as a fine silk thread of 15 inches. When the thread of silk was dipped in fine sealing wax, it was equal to the pure lac, if six inches long, or four times its length. If we measure their excellence by the intensities with which they insulate, lac is three times better than the dry thread, and twice as good as the thread dipped in sealing wax: so that a fibre of silk, even when included in the lac, diminishes its insulating power. We also learn, that the dissipation along these substances is not entirely owing to moisture condensed or adherent on their surfaces, but to a small degree of conducting power. We have repeated many of these experiments, and find that the conducting power of silk thread depends greatly on its colour. When of a brilliant white, or if black, its conducting power seems to be the greatest, and a high golden yellow, or a nut brown, seemed to be the best insulators; doubtless the dyeing drug is as much concerned as the fibre.

Glass, even in its dryest state, and in situations where moisture could have no access to it, *viz.* in vessels containing caustic alkali dried by red heat, or holding fresh made quicklime, appeared in our experiments to be considerably better than silk; and where drawn into a slender thread, and covered with gum lac (melted), insulated when three times the length of a thread of lac; but we found at the same time, that extreme fineness was necessary, and that it dissipated in proportion the square of its diameter. It was remarkably hurt by having a bore, however fine, unless the bore could also be coated with lac. Human hair, when completely freed from every thing that water could wash out of it,

and then dried by lime, and coated with lac, was equal to silk. Fir, and cedar, and larch, and the rose-tree, when split into filaments, and first dried by lime, and afterwards baked in an oven which just made paper become faintly brown, seemed hardly inferior to gum lac.

The *white woods*, as they are called, and mahogany, were much inferior. Fir baked, and coated with melted lac, seems therefore the best support when strength is required. The lac may be rendered less brittle by a minute portion of pure turpentine, which has been cleared of water by a little boiling, without sensibly increasing its conducting power. Lac, or sealing wax, dissolved in spirits, is far inferior to its liquid state by heat.

These observations may be of use for the construction of electrical machines of other electrics than glass.

We have now given a comparison of the hypothesis of Mr. Æpinus with the chief facts observed in electricity, diversified by every circumstance that seemed likely to influence the result, or which is of importance to be known. We trust that the reader will agree with us in saying that the agreement is as complete as can be expected in a theory of this kind; and that the application not only seems to explain the phenomena, but is practically useful for directing us to the procedures which are likely to produce the effect we wish. Thus, should our physiological opinions suggest that copious transference of fluid is proper, our hypothesis points out the most effectual and the most convenient methods for producing it. We learn how to constipate the fluid in a quiescent state, or how to abstract as much of it as possible from any part of a patient; we can do this even in the internal parts of the body. We had once an opportunity of seeing what we thought the cure of a paralysis of the gullet. Electricity was tried, first in the way of sparks,

and then small shocks taken across the trachea. These could not be tolerated by the patient. The surgeon wished to give a shock to the œsophagus without affecting the trachea. We recommended a leaden pistol bullet at the end of a strong wire, the whole dipped in melted sealing wax. This was introduced a little way, we think not more than three inches, into the gullet, which the palsy permitted. A very slight charge was given to it in a few seconds; and the first shock produced a convulsion in the muscle, and the second removed the disorder completely. Here the ball formed the inner, and the gullet the outer, coating of the little Leyden phial.

Notwithstanding the flattering testimony given by the great conformity of this doctrine with the phenomena, we still choose to present it under the title of a hypothesis. We have never seen the electric fluid in a separate state; nor have we been able to say in what cases it abounds, or when it is deficient. After what we have seen in the late experiments of that philanthropic philosopher Count Rumford on the production of heat by friction, we think that we cannot be too cautious on what grounds we admit invisible agents to perform the operations of Nature. We think that all must acknowledge that those experiments tend very much to stagger our belief in the existence of a fluid *sui generis*, a fire, heat, caloric, or what we please to call it; and all will acknowledge, that no better proofs can be urged for the existence of an electric fluid.

225. Accordingly, many acute and ingenious persons have rejected the notion of the existence of an electric fluid, and have attempted to shew that the phenomena proceed not from the presence of a peculiar *substance*, but from peculiar *modes*; as we know that sound, and some concomitant motions and other mechanical appearances, are the results of the elastic undulations of air; and as Lord Bacon and others have explained the effects of fire by elastic undulations of the integrant particles of tangible matter.

We have seen nothing, however, of this kind that appears to give any explanation of the motions, pressures, and other mechanical appearances of electricity. We peremptorily require, that every doctrine which claims the name of an explanation, shall be perfectly consistent with the acknowledged laws of mechanism; and that the explanation shall consist in pointing out those mechanical laws of which the facts in electricity are particular instances. It is no difficult matter to present an intricate or complex phenomenon to our view, in such a form, that it shall have some resemblance to some other complex physical fact, more familiar, perhaps, but not better understood. The specious appearance of similarity, and the more familiar acquaintance with the other phenomenon, dispose us to consider the comparison as a sort of explanation, or, at least, an illustration, and to have a sort of indolent acquiescence in it as a theory.

But this will not do in the present question: For we have here selected a particular circumstance, the observed motions occasioned by electricity, and called *attractions* and *repulsions* —a circumstance which admits of the most accurate examination and comparison with any explanation that is attempted. In such a case, a vague picture would speedily vanish into air, and prove to be nothing but figurative expressions.

226 Many philosophers, and among them some respectable mathematicians, have supported the doctrine of Du Fay, Symmer, Cigna, &c. who employ two fluids as agents in all electrical operations. It must be granted that there are some appearances, where the explanation by means of two fluids seems, at first sight, more palpable and easier conceived. But whenever we attempt to obtain *measures*, and to say what will be the precise kind and degree of the action, we find ourselves obliged to assign to the particles of those fluids actuating mechanical forces precisely equivalent to those assigned by Æpinus to his single fluid. Then we have to add some mysterious unexplained connections,

both with each other and with the other particles of tangible matter. If we except Mr. Prevost, in his *Essai sur les Forces Magnetiques et Electriques*, we do not recollect an author who has ventured to subject his system to strict examination, by pointing out to us the laws of action according to which he conceives the particles influence each other. We shall have a proper opportunity, in the article MAGNETISM, to give this author's theory the attention it really merits. We venture to say, that all the chemical theories of electricity labour under these inconveniences, and have acquired their influence merely from the inattention of their partisans to the laws of mechanical motion, and require, in order to reconcile them with those laws, the adoption of powers similar to Æpinus's attractions and repulsions. Slight resemblances to phenomena, which stand equally in need of explanation, have contented the partisans of such theories, and figurative language and metaphorical conceptions have taken place of precise discussion. It would be endless to examine them all.

227. The most specious of any that we know was publicly read in the university of Edinburgh by the late Mr. James Russel, Professor of natural philosophy; a person of the most acute discernment, and an excellent reasoner. It was delivered to his pupils, not as a *theory*, but as a *conjecture*, founded on Lord Kames's theory of spontaneous evaporation, which had obtained a very general reception; a conjecture, said the Professor, founded on such resemblances as made a similarity of operation very probable, and was an incitement and direction to the philosopher to a proper train of experimental discussion. We say this on the authority of his pupils in the years 1767, 1768, and 1769, and of some notes in his own hand writing now in our possession.

Mr. Russel considered the electrical phenomena as the results of the action of a substance which may be called the *electrical fluid*, which is connected with bodies by attractive

and repulsive forces acting at a distance, and diminishing as the distance increases.

Mr. Russel speaks of the electric fluid as a compound of several others; and, particularly, as containing elementary fire, and deriving from it a great elasticity, or mutual repulsion of its particles. This, however, is different from the elasticity, or mutual repulsion of the particles of air, because it acts at a distance; whereas the particles of air act only on the adjoining particles. By this constitution, bodies containing more electric fluid than the spaces around them repel each other.

The particles of this electric fluid attract the particles of other bodies with a force which diminishes by distance.

The characteristic ingredient of this fluid is *ELECTRICITY properly so called.* This is united with the elastic fluid by chemical affinity, which Mr. Russel calls *elective attraction*, a term introduced into chemistry by Dr. Cullen and Dr. Black. This extends to all distances, but not precisely by the same law as the mutual repulsion of the particles of the other fluid, and in general, it represses the repulsions of that fluid while in this state of composition. This *electricity*, moreover, attracts the particles of other bodies, but with certain elections. Non-electric or conducting bodies are attracted by it at all distances; but electrics act on it only at very small and insensible distances. At such distances its particles also attract each other.

By this constitution, the compound electric fluid repels its own particles at all considerable distances, but attracts at very small distances. It attracts conducting bodies at all distances, but non-conductors, only at very small distances. The phenomena of light and heat are considered as marks of partial decomposition, and as proofs of the presence of elementary fire in the compound: the smell peculiar to electricity, and the effect on the organ of taste, are proofs of decomposition and of the complex nature of the fluid.

Bodies (conductors) containing electric fluid, repel each ther at considerable distances, but, if forced very near, at-ract each other. Electrics can contain it only in consequence f the *electricity* in the compound. Part of this electricity ust be attached to the surface in a non-elastic state; be-ause when it is brought so near as to be attracted, its par-cles are within the spheres of each other's action, and this edoubled attraction overcomes the repulsion occasioned by s union with the other ingredient; and the electric fluid partly decomposed, and the *electricity*, properly so called, dheres to the surface of the electric, *as the water of damp air dheres to a cold pane of glass in our windows.* Also, by this nstitution, electric fluid may appear in two states; elastic, ke air, when entire; and unelastic, like water, when part-decomposed by the attraction of electrics.

Electricity may be forced into this unelastic union by va-ous means; by friction, which forces the electric fluid con-ined in the air into close contact, and thus occasions this ecomposition of the fluid and the union of its *electricity* with ie surface. This operation is compared by Mr. Russel to ie forcible wetting of some powders, such as lycoperdon, hich cannot be wetted without some difficulty and mecha-cal compression; after which it adheres to water strong-. It may be thus united in some natural operations, as observed in the melting and freezing of some substances contact with electrics; and it may be thus forced into nion by means of metallic coatings, into which the electric uid is forced by an artful employment of its mutual repul-ons. This operation is compared to condensation of the oisture of damp air by a cold pane of the window; and e evacuation of the other side of the coated pane is com-ared to the evaporation of the moisture from the other side the window pane, in consequence of the heat which must nerge from the condensed vapour. We find in the Pro-ssor's notes above-mentioned, many such partial analogies, nployed to shew the students that *such* things are seen in

the operations of Nature, and that his conjecture merits attention.

The intelligent reader will see that the general results of this constitution of the electric fluid will tally pretty well with the ordinary electrical phenomena; and, accordingly, this *conjecture* was received with great satisfaction. We remember the being much pleased with it, as we heard it applied by Mr. Russel's pupils, many of whom will recollect what is here put on record. But the attentive reader will also see, that all this intricate combination of different kinds of attraction and repulsion is nothing but mere accommodations of hypothetical forces to the phenomena. How incomparably more beautiful is the simple hypothesis of Æpinus, which, without any such accommodations, tallies so precisely with all the phenomena that have yet been observed? Here no distinction of action is necessary, and all the varieties are consequences of a circumstance perfectly agreeable to general laws; namely, that the internal structure of some substances may be such as obstructs the motion of the electric fluid through the pores—Nothing is more likely.

228. Several years after the death of the Scotch Professor in 1773, a theory very much resembling this acquired great authority, being proposed to the philosophers by the celebrated naturalist Mr. de Luc. This gentleman having long cultivated the study of meteorology with unwearied assiduity and great success, and having been so familiarly conversant with expansive fluids, and the affinities of their compounds, was disposed to see their operations in almost all the changes on the surface of this globe. Electricity was too busy an actor in our atmosphere to escape his particular notice. While the mechanical philosophers endeavoured to explain its effects by accelerating forces attracting and repelling, Mr. de Luc endeavoured to explain them by means of the expansive properties of aeriform fluids and gases, and by their chemical affinities, compositions, and de-

compositions. He had formed to himself a peculiar opinion concerning the constitution of our atmosphere, and had explained the condensation of moisture, whether of steam or of damp aeriform fluids, in a way much more refined than the simple theory of Dr. Hooke, *viz.* solution in air. He considers the compound of air and fire as the *carrier* of the water held in solution in damp air, and the fire as the general carrier of both the air and the moisture. Even *fire* is considered by him as a *vapour*, of which *light* is the *carrier*. When this damp air or steam is applied to a cold surface, such as that of a glass pane, it is decomposed. The water is attracted by the pane by chemical affinity, and attaches itself to the surface. The fire, thus set at liberty, acts on the pane in another way, producing the equilibrium of temperature, and the expansion of the pane. Acting in the same manner on the moisture which chances to adhere to the other side, in a proportion suited to its temperature, it destroys their union, enters into chemical combination with the moisture, and fits it for uniting with the air on the other side, or carries it off. Having read Mr. Volta's theory of *electric influences*, by which that philosopher was enabled to give a scientific narration and arrangement of the phenomena of the *electrophorus* newly invented by himself, and which is called an explanation of those phenomena, Mr. de Luc imagined that he saw a close analogy between those *influences* on the plates of the electrophorus and the *hygroscopic* phenomena of the condensation and evaporation of moisture. In short, he was struck with the resemblance between the condensation of moisture on one side of a glass pane, and its evaporation from the other; and the accumulation of electric fluid on one side of a coated pane, and the abstraction of it from the other. Subsequent examination pointed out to him the same analogy between all other *hygroscopic* and *electric* phenomena.

He therefore immediately formed a similar opinion concerning the electric operations. It may be expressed briefly as follows:

229. The electrical phenomena are the operations of an expansive substance, called the *electric fluid*. This consists of two parts: 1. *Electric matter*, which is the gravitating part of the compound; and *electric deferent fluid*, or *carrying fluid*, by which alone the electric matter seems to be carried from one body to another. The resemblance between the hygroscopic and electrical phenomena are affirmed to be*,

1. As watery vapour or steam is composed of fire, the deferent fluid, and water, the gravitating part, so *electric fluid* is composed of the *electric deferent fluid*, and *electric matter*.

2. As *vapours* are partly decomposed when too dense for their temperature, and then their *deferent fluid* becomes free, and shews itself as *fire*; so *electric fluid* that is too dense is decomposed, and its *deferent fluid* manifests itself in the *phosphoric* and *fiery* phenomena of *electricity*.

3. As *fire* quits the *water* of *vapour*, to unite itself with a body less warm; so the *electric deferent* quits the *electric matter*, in part, to go to other bodies which have proportionally less of it.

In this analogy, however, there is a distinction. *Fire*, in quitting the *water* in *vapour*, remains actuated by nothing but its expansive force; remains free, and extends itself till the equilibrium of temperature is restored; but the *electric deferent*, when disengaged from *electric matter*, in order to restore its peculiar equilibrium, is actuated by *tendencies* to distinct bodies, and acts by this *tendency* in thus restoring the *electric equilibrium*; and it is only in consequence of this *tendency* that it quitted the *electric matter*. This *tendency* is then directed to some body in the vicinity.

4. As the *fire* of *vapour* pervades all bodies, to restore the *equilibrium of temperature*, deposing the *water*; so the *electric deferent* quits the *electric matter*, to restore the *electric equilibrium* in an instant, and for this purpose pervades all

* See *Idées sur la Meteorologie*, § 266, &c.

bodies, depositing on them the *electric matter* which it carried, but differently, according to their natures.

5. As *fire* and *water*, while composing *vapour*, retain their *tendencies* and *affinities* by which they produce the *hygroscopic* phenomena: so the ingredients of the *electric fluid*, even in their state of union, retain their *tendencies* and *affinities*, which produce the greatest part of the *electric phenomena*.

6. In particular, the *electric matter* retains its *tendencies* and *affinities*; and farther, the *electric affinities* are, like the *hygroscopic*, without any choice.

Here, however, there is a farther distinction. The affinities of *water* respect only *hygroscopic* substances; but those of *electric matter* respect all substances, and therefore respect the common atmospheric fluids.

7. When *fire* quits the *water* of *vapour*, to form the *equilibrium of temperature*, it remains in the place where *vapour* most abounds, but is partly *latent*, not exerting its powers; so in the restoration of the equilibrium of the *electric deferent* among neighbouring bodies, those which have proportionally most *electric matter* also retain most *deferent fluid*, but in a *latent* state.

8. As two masses of *vapour* may be in *expansive equilibrium* (which others call balancing each others elasticity) although the *vapours* contain very different proportions of *fire* and *water*; so two masses of *electric fluid* may be in *expansive equilibrium*, although one contains much more *electric matter* in the same bulk, provided that the *electric deferent* be also more copious.

The chief distinction that mingles with these analogies is, that the *affinity* of *water* to *hygroscopic* substances operates only in contact, whereas *electric matter* tends to distant bodies; and these distances are very different in regard to different bodies.

Such is the resemblance which has appeared so strong to Mr. de Luc. It is evidently the same which furnished the conjecture to Mr. Russel, and which he considered mecha-

nically, in order to explain the phenomena of electric motions to students of mechanical philosophy. The only resemblance seems to us to appear in the condensation of moisture contained in damp air.

Mr. de Luc, led by the habits of his former studies, attempts to explain every thing by the relations which were most familiar to him, *affinities* and *expansive forces*. Let us attend a little to the manner in which he explains one or two of the most general facts.

230.—*First, The conditions of conductors and non-conductors.*

This distinction depends on the differences in the *tendency* to distant bodies: there are great differences in these distances according to the nature of the bodies; and from this arise great differences of phenomena, independent of insulation or non-insulation, which are only the sensible distinctions of these classes of bodies. *Electric matter* tends to *conductors* at great distances; but having reached them, it does not adhere, and remains free to move round them, being dragged by the *deferent fluid;* but its tendency to *non-conductors* is only at small and insensible distances; and having come into contact, it adheres, and can no longer be dragged by the *deferent fluid.*

Hence the operation of *conductors* and *non-conductors;* and there is no other foundation for the notion of *idio-electrics* and *non-electrics*, or electrics by communication. A part of a *non-conductor* takes as much *electric* matter as it can from the substance furnishing it; but cannot communicate it to another part, except very slowly; therefore, to communicate it to the whole surface, we must cover it with a conductor. (Surely this is a distinction in the body, independent of the distance of mutual tendency!)

Hence, too, the property of *non-conductors* by which the electric fluid is *benumbed (engourdi)* or cramped; therefore we can accumulate a great deal in them; and it will remain long being *benumbed;* and if it be determined to quit them

at once, the current will be much more dense than when quitting an equal conducting surface.

Since *conductors* do not fix the *electric fluid*, it *must circulate round them.* It is urged to this motion by its *expansive power*, by which it would disperse from a body with inconceivable velocity, and perhaps the rapidity of its motion would decompose it, and cause some *light* to emerge; but it is at the same time impelled by its *tendency* to bodies. Thus, by these two forces, it runs to a *conducting body*, and must circulate round it as the planets do round the sun. In this circulation, if it come to any great projection, it cannot follow the outline, because so abrupt; it therefore flies off at all points and protuberances. It will be the more difficult to keep to an abrupt outline as the stratum in circulation is more copious or deeper, because a greater mass is with difficulty turned round a sharp angle. It is more inclined to escape if another body be near, and it immediately becomes a satellite to that body.

Thus all bodies get a share of electric fluid, circulating round conductors, and *benumbed* or *cramped* in *non-conductors.* Bodies of this last class receive their portion by the air as *hygroscopic substances* receive their water by the *fire.*

All the differences in the tendencies to bodies proceed from the *electric matter.* The *deferent fluid* follows other laws; namely, 1. Its tendency to all substances is greater than that of the *electric matter* to any one. 2. The tendency (and also that of the *electric matter*) is always from the body which contains most of it, to that which contains least. 3. The body which contains most of the one also contains most of the other. 4. The *deferent fluid* has a particular affinity (chemical) with the *electric matter.* 5. All these tendencies are lessened by an increase of distance. 6. The *electric matter*, when composing *electric fluid*, has more or less *expansive force* as it is united to more or less *deferent fluid.*

Explanation of Charged Plates.

231. Mr. de Luc says (§ 286), that his SYSTEM was suggested by Volta's *Theory of electric Influences.* These (says he) had been pretty well generalised before, but with little improvement to the science, till Mr. Volta discovered a circumstance which, in his opinion, connected by a general theory many phenomena which had formerly no observed relation to any thing. This was, that *when a body electrified positively brings a neighbouring body communicating with the ground into the negative state, its own positive electricity is weakened while it remains in that neighbourhood, but is recovered when the other body is removed.* "Such is the distinguishing law of Mr. Volta's theory, which brings all the phenomena of electric influences under his theory, beginning with those of coated glass, which were formerly so obscure, because they were not referred to their true cause, &c.

"My SYSTEM (Mr. de Luc says) concerning the nature of the *electric fluid* explains the laws of Mr. Volta's theory; and of consequence explains, like it, all the phenomena which it comprehends: but it reaches much farther, seeing that more general laws comprehend a greater number of phenomena.

"In the phenomena of coated glass, I plainly saw one of the procedures of *watery vapour.* Suppose a glass pane, moistened on both sides, and having the temperature of the surrounding bodies. Suppose that warmer *vapour* comes to one side. It is condensed on the surface; that is, it is decomposed, the *water* adheres to the surface, and the *fire* penetrates the glass, heats it, and increases the evaporation from the other side, by entering into combination with the *water*, and carrying it off with it. More *vapour* is condensed on the side A; more *fire* reaches the side B, and carries off more *water.* But as this happens only because the *fire* also raises the *temperature* of the pane, it is evident that the condensation on the side A, and the evaporation from B,

must gradually slacken, and the *maximum of accumulation* in A, and of evaporation from B, will take place when the temperature of the pane is the same with that of the hot *vapour*.

" The electrical phenomena of coated glass are perfectly similar. The *electric fluid* reaches the side A, is decomposed, and the *electric matter* is there *benumbed* and fixed The *deferent fluid* penetrates the pane, and carries off the *electric matter* from the side B. This goes on, but slackens; and the maximum of accumulation and evacuation obtains when the side A has acquired the same intensity of electricity with the charging machine. More is accumulated in A than is abstracted from B; because B is farther from the source (he might have added, that part of the fire is expended in raising the temperature of the pane): but the accumulation is inactive, because the *electric matter* is *benumbed* and fixed. Though the *electric matter* is much diminished in B, yet the *electric fluid* in its coating has as much expansive force as that of the ground; because it has a surplus of deferent fluid. The aboslute quantity of *electric matter* in both sides is somewhat augmented."

232. This explanation of the Leyden phial comprehends the whole of Mr. de Luc's theory; and the constitution of the electric fluid, and its various affinities, expansive powers and tendencies, are all assigned to it in subserviency to this explanation, or deduced from those phenomena. As the author, in all his writings, claims some superiority over other naturalists for more general and comprehensive views, and for more scrupulous attention to precision and measurement, and particularly for more solicitude that no natural agent be omitted that has any share in the proceedure,—he surely will not be offended, although we should state such difficulties and objections as occur to us in the consideration of this System (as he chooses to call it) of electricity.

We wish that it had been expressed in the plain and precise language of mechanical and chemical science; for he reasons entirely from the nature of expansive forces, tenden-

cies, and affinities. His language will appear to some readers, as it does to us, rather to express the conduct of intelligent beings, acting with choice, and for a purpose, than the laws of lifeless matter. His account would have been less agreeable, it is true, but more instructive, and less apt to be mistaken. Metaphorical language is seldom used without the risk of metaphorical conceptions; and the reader is very apt to think that he has acquired a notion of the subject, while he is really thinking of a thing of a different nature. We apprehend that a great deal of this happens in this instance, and that when the narration is stripped of its figurative language, it will be found without that connection and analogy which it seems to possess.

We also wish that the explanation had been derived from some well-established principle. The whole of it is *professedly* founded on a resemblance between the *phenomena* of electricity and some things said of watery vapour; but these are not the *phenomena* of watery vapour, but Mr. de Luc's *hypothesis* (he will pardon us the term, which we prefer to *system*) concerning *watery vapours*. We do not think it philosophical to explain one hypothesis by another. Our illustrious countrymen Bacon and Newton, disapproved of this practice; and their rules of philosophising have still currency among philosophers. Explanation, in our opinion, is the pointing out some acknowledged general fact in nature, and shewing that the particular phenomenon is an example of it. We do not see this in Mr. de Luc's explanation; because we do not see the *facts* in the case of watery vapours to which the *phenomena* of electricity are said to have a resemblance. The phenomena we mean are chiefly the *motions*, and the *transferences* of the powers producing such motions; we do not speak of the *light*, and some other phenomena, because Mr. de Luc does not speak of them in this explanation. We shall even admit the *transference* as a *phenomenon*, although we do not see any substance transferred: but we see a power of producing certain motions,

where that power did not formerly appear; and the appearance of this power is all the authority adduced, even by Mr. de Luc, for the transference. We must now add, that the electric phenomena, which Mr. de Luc calls like the phenomena of watery vapour, are all *suppositions*; and that therefore the explanation is a system of suppositions, *framed* so as to be like the system of watery vapour. For Mr. de Luc will grant, that on the one hand, we see nothing like the water in the electric phenomena; and, on the other hand, there is nothing in watery vapour like the motions of the electrometers, which are the only PHENOMENA from which Mr. de Luc professes to reason.

We also fear that the very curious experiments of Count Rumford on the melting of ice, and the propagation of heat through liquids, will oblige Mr. de Luc to change the tasks of the ingredients, both of *vapour* and of *electric* fluid. *Water*, and not *fire*, seems to be the *carrier* or *deferent fluid*; and we think that Franklin and Æpinus have made it highly probable that electricity, and not air, is the carrier.

We have also great difficulty in conceiving (indeed we cannot conceive) how the *deferent fluid*, from which the *electric matter* has been detached by its superior *affinity* with the side A, can overcome the *same superior affinity* of the *electric matter* with the side B (*), and carry it off; how the *deferent fluid* penetrates the non-conducting pane, in order to carry off the *electric matter* in the form of *fluid*; and how it cannot do this, except by means of a *conducting canal*, into which *it is expressly said that it does not penetrate*. It must not be said that it runs along the surface of this canal: for the smallest wire will be a sufficient conductor, covered a foot thick with sealing wax. This indeed, according to Mr. de Luc, allows the deferent *fluid* to pass; but it must also, according to him, strain it pretty clear of all *electric matter*.

* We may here ask, How comes there to be such a quantity of electric matter already lodged in B?—Is it benumbed? or in what state is it?

For we cannot help thinking, that the process (although purely ideal) has a closer resemblance to what we should observe in a stream of muddy water poured on a strainer, both sides of which are previously foul. If we were disposed to amuse ourselves with a figurative hypothesis, we could give one on the principle of filtration that is very pretty, and pat to the purpose, of glass coated, and charged, and discharged by conducting canals.

With respect to the suggestion of this theory by Volta's theory of electric influences, and the ignorance of naturalists before that time of the true state of things, we must observe, that Mr. Russel proposed the same analogy to the consideration of his hearers many years before; and it was very generally known. The electric influences had been fully detailed by Æpinus and Wilcke in 1759, and applied with peculiar address and force of evidence by Mr. Cavendish before 1771; and they were described nearly in the same way by Lane, Lichtenberg, and others.

And with respect to Mr. Volta's general principle, which Mr. de Luc prizes so highly, and by which he explains every thing, we must observe, that *it is not true as a phenomenon in electricity;* but, on the contrary, *the positive state of a body is rendered stronger, or more remarkable, by inducing the negative state on a neighbouring body.* See § 52. and 66. Mr. Volta was misled by the appearances of the electrophorus, which had engaged all his attention, and modelled all his notions on these subjects. His observations had been confined to disks; and though these are excellent instruments for producing very sensible effects, they are quite unfit for examining the general nature of electric influences. Even without much knowledge of dynamics, a person must perceive that the action of their different parts on the electrometer may be very different, by reason of their different positions and distances from it. Besides, the electrometers of the apparatus described by Mr. de Luc in sect. 440, &c. *did not* indicate the real condition of the disks to which they

were attached, but the condition of the remote ends of overcharged conductors of considerable length. Therefore, although all the electrometers fell lower when the other group of disks was brought near, the positive state of the nearest disk was greatly augmented. The most unexceptionable apparatus for this purpose would be a row of polished balls on insulating stands, placed in contact, the whole charged positive; and when another such group, or a long body, is brought near, let the balls be separated at once, and examined apart by a very small electrometer, made in the form of our figure 8. We presume to say that, if the other group is properly managed, and made to communicate thoroughly with the ground, the positive electricity of the balls nearest to it will be found greatly augmented, and that every one of them will be found in that precise state of electrification that is pointed out by the Æpinian theory. Mr. de Luc has made and narrated the experiments with the disks, and the curious figures observed by Lichtenbergh, with great judgment and fidelity; and they are classical and valuable experiments for the examination of the theory. We may here mention a very neat way of executing the apparatus of balls, which was practised by a young friend, who was so kind as to make the experiments for us, when our thoughts were turned to Mr. de Luc's theory. Each ball was mounted on a slender glass rod varnished. The lower end of the stalk was fixed in a little block of wood which had a square hole through it, by which it slided steadily along a horizontal bar of mahogany, supported at the ends about an inch from the table. The balls were made to separate at once, and equally, from each other, by a chequer-jointed frame, such as is seen in the toyshops, carrying a company of foot soldiers, who open and close their ranks and files by pulling or pushing the ends of the frame. Taking out the pins of the middle joints of this chequered frame-work, and widening the holes for receiving the glass stalks, it is plain that all the balls will separate at once, in the very state of elec-

tricity in which they were when in the neighbourhood of the non-insulated group. This apparatus consisted of six balls We found the ball next the other group much more strongly positive than before bringing that group near; and it was generally the third ball which seemed equally electric in both situations. We added nine balls more, connecting the whole by a similar contrivance; and found it a most instructive apparatus for the theory of the distribution of the electric fluid. We wish that it had occurred to us when the § 62, &c. were under consideration.

With respect to the condition in which the electric matter is said to be lodged in the side A of the coated pane, where Mr. de Luc says that it is fixed, *engourdi* in the *non-conducting surface* (which condition Mr. de Luc considers as characteristic of such substances), we must say that the description of its state is by no means agreeable to what we have observed. The powers of this *electric matter* are no more *benumbed* or enervated (it is a very unphilosophical phrase), that if it were in a *conducting* body at the same distance from the opposite coating. If coatings be applied to a block of glass of two or three inches in thickness, and if the electrification be so moderate that it would not fly from the one coating to the other when the glass is removed—no sensible difference will be found between the electricity of the two coatings with or without the glass. The electric matter in the side A has not its powers *engourdi*; they are *balanced* by the powers of the side B.

But how will Mr. de Luc explain the charging a pane negatively? How will he bring off a quantity of *electric matter*, greater (according to his own account) than what will be *benumbed* on the other side? Nay, we must ask, where does he find it? Is there a quantity already *benumbed* there? What is to revive it?

Let us now consider a little the constitution of the ingredients of this electric fluid, by which all these things are brought about. And in doing this, let us banish, when pos-

sible, all figurative language; and, in the precise and dry phraseology of dynamics, let us speak of the motion of single particles of the *electric fluid*, *deferent fluid*, and *electric matter*. By *expansive power*, must certainly be meant such a power as that by which air, gases, inflamed gunpowder, steam, and the like, enlarge their bulk, and which is clearly manifested as a mechanical pressure, by bursting vessels, impelling bullets or pistons, &c. as well as by the actual enlargement of the bulk of the fluid. We have no other indications of its being a *force;* and therefore our notions of its mode of acting must be derived solely from what we *understand* of this power in air or the other fluids. Newton's *Principia* are our authority for saying, that all that we know of it is, that it acts as a number of corpuscles would act, which repel each other with a force inversely proportional to their distances; this action not extending beyond the adjoining corpuscle, not even to the second. We know a good deal of the propagation of pressure and progressive motion through such a fluid, when it is confined in a vessel, or system of vessels, of any form, and some few simple circumstances which take place in the elastic undulations which may be excited and propagated through it. We have but a *very indistinct notion* of the motions which one mass of such a fluid will produce in another mass, when both are at liberty to expand. But we are certain that it will be like the motion of two masses of air blown or driven against each other. Now these electric fluids, by their expansive powers, must act like those others with which we are more familiarly acquainted. And here we venture to say, that the appearances in electricity are so far from being like these, that we cannot imagine any thing more remarkably different. We shall mention but one thing. Every mark that we have for the presence of *electric fluid* obliges us to grant, that in an overcharged body it is crowded into the external surface, so that the quantity has little or no relation to the quantity of matter in any body but merely to its surface. This is

quite unlike air, or any other expansive fluid, which is uniformly distributed through the whole space comprehended by the surface which bounds it. We never saw any thing like streams of this *electric fluid*, impelling or any way acting on each other, except in the transference by sparks; and there it was indeed like the motions of air, for it was not *electric fluid*, nor *electric matter*, but *electrified air*.

Let us next consider the *tendencies* by which the relations of these expansive fluids to other bodies are produced, and the electric motions are said to be explained. We observe that Mr. de Luc avoids the use of the words *attraction* and *repulsion*, so much employed by the British philosophers. He considers these tendencies as determinate impulsions, and adopts the doctrine of *Le Sage of Geneva*, who has not only laid Newton under great obligations, by a mechanical explanation of gravity, but has also explained expansion, elasticity, chemical affinity, and all specific tendencies, to the satisfaction of the most eminent mathematicians. To such only Mr. de Luc professes to address himself, who are not contented with a doctrine which supposes bodies to act where they are not. But, unfortunately, Mr. le Sage has never obliged the world with this explanation. We are not most eminent mathematicians; but we are able to prove, that Mr. le Sage's favourite theorem, mentioned by Mr. de Luc in § 157, 158. as demonstrated by Mr. Prevost, the editor of *Lucrece Neutonien*, is a complete dereliction of the first principles of Mr. le Sage, and is also incompatible with mechanical laws. Mr. de Luc should have given a demonstration of the theorem on which all his system rested; otherwise it is only reviving "*dixit philosophus, ergo verum.*"

But let us see what these tendencies perform. Mr. de Luc says, that the fluid, setting out from a body by its expansive power, would move in a straight line with inconceivable velocity, and would immediately desert even this globe were it not deflected by its tendency to other bodies. We do not see whence this immense velocity is derived. But

let it go off; it is deflected from its rectilineal course by its tendency to some conducting body, which it reaches, but cannot, or does not, enter; and therefore *must continually circulate round it, as the planets circulate round the sun,* following its outline, if not too abrupt, but flying off from all points in the direction of the axis of the point, &c. Here we are at home; for this is a plain dynamical problem of central forces. All that we shall say on this head is, that Mr. de Luc has certainly not considered the planetary motions with attention, when he hazarded this very comprehensive proposition. If he will take the trouble to do this, he will see that every part of it is inconsistent with the acknowledged laws of mechanism, and that the motions are absolutely impossible. Besides, we know that it will not fly off from a hundred points placed together, which is a still more abrupt outline, if they do not project beyond the brim of a pit in which they stand; yet this pit only makes the outline more abrupt. We farther believe, that no person can form to himself any distinct notion of such circulations round every conducting body; they will be more numerous, and infinitely more confused and jarring, than all the vortices of Des Cartes. How can such motions take place round a bunch of brass wire buried in sealing wax? Yet he must grant that they really happen there; or what prevents the *electric fluid* from being *strained* clear of all electric matter in passing through the air?

We would also ask, why the tendency is always *from the body containing most of the fluid to that containing least?* It is not enough to say that it is so; this would only be contriving a thing to suit a purpose; a reason should be given if we pretend to explain. Now the tendency to a distant body *is to the matter* in that body, without any relation to the fluid in it, or in the body from which it came.

On the whole, we cannot think this theory is any thing but telling a story of ideal beings, in very figurative language, which gives it some animation and interest. The

different affinities, tendencies, and powers, are only ways of expressing certain *supposed* events, and suited to those events; but it gives no explanation of the *observed mechanical phenomena* of electricity, shewing from acknowledged principles that they must be so.

What a difference between this laboured and intricate mechanism, and the simple, perspicuous, and distinct theory of Æpinus! Even Mr. Russel's explanation is more intelligible, and more applicable to the *motions* which are really observed. That gentleman saw the necessity of considering them as the subjects of *mechanical discussion*, and that all that was wanted was to find out what law of distant action would tally with the phenomena. The Scotch philosopher was careful to warn his hearers that he only proposed a *conjecture*. The Swede calls his performance *Tentamen Theoriæ*, &c. and begins and concludes it with expressly saying, that it is only a *hypo thesis*. The English nobleman calls his dissertation an *Attempt* to explain some of the phenomena, &c. None of these philosophers call their works a SYSTEM, which comprehends all theories, whether that of Volta or of any other successful inquirer.

We hope to be excused for treating so largely of this subject. It struck us as a very proper example of the bad consequences of indulging in figurative language. It must be very seducing, when so scrupulous and so eminent a philosopher as Mr. de Luc is led astray by it.

233. WE conclude this long article by observing, that whatever may be the fate of Mr. Æpinus's *hypothetical theory*, his classification of the facts, and his precise determination of the *mechanical phenomena* to be expected from any proposed situation and condition of the substances, will ever remain, and be an unerring direction in future experiments; and the whole is an illustrious specimen of ingenuity, address, and good reasoning. We hope to make this still more evident, when we apply it to the quiet and manageable phenomena of MAGNETISM.

APPENDIX;

CONTAINING AN ABSTRACT OF MR. COULOMB'S EXPERIMENTS.

234. MR. COULOMB in the *Mem. de l'Acad. de Paris* for 1786, relates several experiments made for ascertaining the disposition or distribution of the electric fluid in an over charged body. Their general results were,

1. That the fluid is distributed among bodies according to their figure, without any elective affinity to any kind of substance.

For when a ball, or body of conducting matter, and of any shape, is electrified to any particular degree, as indicated by his electrometer, if it be touched by another equal and similar body, similarly situated in respect of the touching points, the electricity is always reduced to $\frac{1}{2}$.

2. In an overcharged conducting body, the fluid diffuses itself entirely along the surface, without penetrating into the interior parts.

The conducting body AB (Plate II. fig. 13.) had pits *a*, *b*, &c. made in various parts of its surface. They were half an inch in diameter, and some of them $\frac{1}{10}$th, others $\frac{2}{10}$ths, others $\frac{3}{10}$ths, &c. in depth. *c* represents the edge of a small circle of gilt paper, $\frac{1}{5}$th of an inch in diameter, fixed perpendicularly on the end of a fine thread of gum lac. The body was electrified and touched with this little electroscope, by setting it flat down on the surface. The circle *c* was then presented to an electrometer which moved 90 degrees by a force not exceeding $\frac{1}{6000}$th of a French grain. When this contact was made with the even surface of the conductor, it was strongly electrified, and particularly when it touched any

eminence, or the ends of long cylinders, &c. The paper being exceedingly thin, and placed in full contact, it may be supposed to bring off with it the quantity of fluid corresponding to that part of the surface, or rather a greater quantity. But when it was made to touch the bottom, even of the shallowest of these pits, it did not affect the electrometer in the least.

He demonstrates the following elementary theorem:

The attraction or repulsion being supposed to be proportional to the inverse of any power m of the distance; that is, being as $\frac{1}{x^m}$: if m be greater than 3, the action of all the masses of fluid which are at a finite distance is nothing in comparison with the action in contact; and therefore the fluid must be uniformly diffused, in the same way as if each particle acted only on the adjoining particles.

But if m be less than 3, for example if m be 2, as seems to be the case in electricity, the action of all the masses at a finite distance is not infinitely small in comparison with the action in contact, and the redundant fluid must go toward the surface, and no redundant fluid will be retained in the interior parts. The demonstration is to this effect.

Let A a BF (Plate II. fig. 14.) be a perfectly conducting body of any shape, and let $d\ a\ e$ be a thin slice separated from the rest by the plane $d\ e$; let $d\ c\ e$ be precisely equal and similar to $d\ a\ e$, and let $a\ b\ c$ be perpendicular to the separating plane; then the action of all the particles in the thin slice $d\ a\ e$ (when estimated in the direction $a\ b$) on the particle b, must balance the action of all the rest of the fluid in the body; for b is supposed to be at rest. Now, as the law of continuity will be observed in *any* distribution of the fluid, *through the whole body*, it is plain that, by taking $a\ b$ sufficiently small, the difference of density at a and at c may be infinitely small; therefore the action of the fluid in $d\ a\ e$ will be infinitely near to an equilibruim with the action of $d\ c\ e$; and the action of the fluid in the rest of the body on

the particle b will be infinitely small. This cannot be, when the action of a mass of fluid at a finite distance is not infinitely small in comparison with the action in contact, unless we suppose that the quantity of fluid at a finite distance is also infinitely small, or nothing; that is, unless the whole redundant fluid is constipated on the surface, and the interior parts are merely saturated.

The preceding propositions are quite analogous to propositions in Mr. Cavendish's dissertation in the Philosophical Transactions for 1771.

235. In the Memoirs of the same Academy for 1787, Mr. Coulomb endeavours to ascertain the density of the fluid in different bodies which touch each other. When the bodies do not differ extremely in magnitude, he determines this by the immediate application of them to the electrometer; but when one is extremely small in comparison with the other, he first determines the force of the large body, and then touches it 20 or 40 times with the small one, till the force of the large body is reduced to $\frac{1}{2}$, $\frac{1}{3}$, $\frac{2}{3}$, &c. The general result was, that when the surfaces of the spheres had the proportion expressed in the first column of the following table, then the density in the small one had the proportion expressed by the numbers of the second column, and never attained the magnitude 2.

1	1.
4	1,08.
16	1,3.
64	1,65.
Infinite	2.

This is extremely different from the proportions which obtain when the two spheres communicate by very long slender canals, which he found exactly conformable to the determinations of the theory: but in Mr. Coulomb's experiments the spheres touched each other, and had no other communication.

He then endeavours to ascertain the density of the fluid in the different parts of the surface of these touching spheres, in order to obtain some experimental knowledge of the distribution. He touched them (while in mutual contact) with the little paper circle, and examined its electricity by his electrometer, and made his estimation, on the supposition that it brought off one-half of the electricity of the touched part.

236. When the globes were equal, he found the density to be 0 in the point of contact, and scarcely sensible till he took the paper 30 degrees from the point of contact. From this it increased rapidly to 60°; slowly from thence to 90°; and from thence to 180° it was almost uniform. The densities were nearly

0	at	0°.
0	—	20°.
1	—	30.
3,72	—	60.
4,78	—	90.
5,03	—	180.

He also found, that the more the globes differed in bulk, the more is the density changed in the small globe, and it is the more uniform in the great one, increasing rapidly from 0, at the point of contact, to about 7°, and beyond this being sensibly uniform*.

Hence we may conclude, that the electricity is diffused with almost perfect uniformity in a globe communicating with another at a great distance by a slender canal (as Mr. Cavendish has demonstrated); while, from the reasoning employed before, it is probable that it is also uniformly diffused all along the canal; and therefore, that the quantities

* A very full account of Coulomb's experiments on the electrical density of two globes in contact; on the distribution of Electricity among several globes placed in contact in a straight line; on the distribution of Electricity over several unequal globes, and on its distribution between a globe and cylinders of different lengths, will be found in the Edinburgh Encyclopædia, Art. ELECTRICITY, vol. viii. p. 452, 457. *Ed.*

in two such globes are very nearly as the diameters, and the densities inversely as the diameters, as Mr. Cavendish demonstrated, on the supposition that the fluid in the canal is incompressible.

He found that a small globe, placed between two equally large ones, shewed electricities of the same kind with that of the other two, when the radius of the great one was not more than five times that of the middle one, but shewed no electricity when the disproportion was greater.

237. When three equal globes were in contact, the density of fluid in the middle globe was $\frac{1}{1,34}$ of that of the other two. A small globe being removed to a very small distance from an overcharged great one, *after having been in contact*, shewed opposite electricity in the fronting point; when a little farther off, it was neutral; and beyond this, it was overcharged.

The diameters being 11 and 8, the fronting point of the small one was negative till the distance was 1; here it was neutral, and when it was removed farther, it was positive. When the diameters were 11 and 4, the small globe was negative till their distance was 2, where it was neutral. When the diameters were 11 and 2, the distance which rendered the small globe neutral in the fronting point was $2\frac{1}{2}$.

All these facts are perfectly conformable to a mathematical deduction, from the supposition that the redundant fluid is spread over the surface, and that the interior points are neutral. If any sort of doubt should remain in the minds of those who are not conversant in such discussions, it must be greatly removed by the fact, that it is quite indifferent whether one or both globes be solid, or be an extremely thin shell.

When an electrified body is touched with a long wire, and by another of equal diameter and length, coated to any thickness with lac or sealing wax, the two wires take off precisely the same quantity of electricity. This was demon-

strated by touching a globe repeatedly till the electricity was reduced to $\frac{1}{2}$.

Hence we must conclude, that the electric fluid does not form active atmospheres around bodies, by the action of whose particles in contact (mathematical or physical) the phenomena of attraction and repulsion are produced, but by the action of the fluid in the body, agreeable to the theory of Æpinus.

Such are the observations of Mr. Coulomb. They are extremely valuable, because they confirm in the completest manner the legitimate consequences of the theory.

We think that the materiality of that which is transferred from place to place in the exhibition of electric phenomena, is greatly confirmed by some observations of Dr. Wilson's in the Pantheon. When a spark was taken from the whole of the long wire extended in that vast theatre, the sensation was so different from a spark which conveyed even a much greater quantity of fluid from a pretty large, but compact, surface, that they could hardly be compared. The last was like the abrupt twitch with the point of a hooked pin, as if pulling off a point of the skin; the spark from the long wire was more like the forcible piercing with a needle, not very sharp, breaking the skin, and pushing it inward. We had this account from the Doctor in conversation. He ascribed it, with seeming justice, to the momentum acquired by the fluid accelerated along that great extent of wire.

MAGNETISM.

238. The knowledge which the antient naturalists possessed of this subject was extremely imperfect, and affords the strongest proof of their ignorance of the true method of philosophising; for there can hardly be named any object of physical research that is more curious in itself, or more likely to engage attention, than the apparent life and activity of a piece of rude unorganised matter. This had attracted notice in very early times; for Thales attributed the characteristic phenomenon, the attraction of a piece of iron, to the agency of a mind or soul residing in the magnet. Philosophers seem to have been contented with this lazy notice of a slight suggestion, unbecoming an inquirer, and rather such as might be expected from the most incurious peasant. Even Aristotle has collected no information that is of any importance. We know that the general imperfection of ancient physics has been ascribed to the little importance that was attached to the knowledge of the material world by the

philosophers of Greece and Rome, who thought human nature, the active pursuits of men, and the science of public affairs, the only objects deserving their attention. Most of the great philosophers of antiquity were also great actors on the stage of human life, and despised acquisitions which did not tend to accomplish them for this dignified employment: but they have not given this reason themselves, though none was more likely to be uppermost in their mind. Socrates dissuades from the study of material nature; not because it was unworthy of the attention of his pupils, but because it was too difficult, and that certainty was not attainable in it. Nothing can more distinctly prove their ignorance of what is really attainable in science, namely, the knowledge of the *laws of nature*, and their ignorance of the only method of acquiring this knowledge, viz. observation and experiment. They had entertained the hopes of discovering the *causes* of things, and had formed their philosophical language, and their mode of research, in conformity with this hopeless project. Making little advances in the discovery of the causes of the phenomena of material nature, they deserted this study for the study of the conduct of man; not because the discovery of causes was more easy and frequent here, but because the study itself was more immediately interesting, and because any thing like superior knowledge in it puts the possessor in the desirable situation of an adviser, a man of superior wisdom; and as this study was closely connected with morals, the character of the philosopher acquired an eminence and dignity which was highly flattering to human vanity. Their procedure in the moral and intellectual sciences is strongly marked with the same ignorance of the true method of philosophising; for we rarely find them forming general propositions on copious inductions of facts in the conduct of men. They always proceed in the synthetic method, as if they were fully conversant in the first principles of human nature, and had nothing to do but to make the application, according to the established forms of

logic. While we admire, therefore, the sagacity, the penetration, the candid observation, and the happy illustration, to be found in the works of the ancient moralists and writers on jurisprudence and politics, we cannot but lament that such great men, frequently engaged in public affairs, and therefore having the finest opportunities for deducing general laws, have done so little in this way; and that their writings, however engaging and precious, cannot be considered as any thing more refined than the observations of judicious and worthy men, with all the diffuseness and repetition of ordinary conversation. All this has arisen from the want of a just notion of what is attainable in this department of science, namely, the laws of intellectual and moral nature; and of the only possible method of attaining this knowledge, viz. observation and experiment, and the formation of general laws by the induction of particular facts.

239. We have been led into these reflections by the inattention of the ancients to the curious phenomena of magnetism; which must have occurred in considerable and entertaining variety to any person who had taken to the experimental method. And we have hazarded these free remarks, expecting the acquiescence of our readers, because the superior knowledge which we, in these later days, have acquired of the magnetical phenomena, were the first fruits of the true method of philosophising. This was pointed out to the learned world in 1590 by our celebrated countryman Chancellor Bacon, in his two great works, the *Novum Organum Scientiarum*, and *De Argumentis Scientiarum*. Dr. Gilbert of Colchester, a philosopher of eminence in many respects, but chiefly because he had the same just views of philosophy with his noble countryman, published about the same time his *Physiologia Nova, seu Tractatus de Magnete et Corporibus magneticis*. In the introduction, he recounts all the knowledge of the antients on the subject, and their supine inattention to what was so entirely in their hands; and the impossibility of ever adding to the stock of useful knowledge,

so long as men imagined themselves to be philosophising while they were only repeating a few cant words, and the unmeaning phrases of the Aristotelian school. It is curious to remark the almost perfect sameness of Dr. Gilbert's sentiments and language with those of Lord Bacon. They both charge, in a peremptory manner, all those who pretend to inform others, to give over their dialectic labours, which are nothing but ringing changes on a few trite truths, and many unfounded conjectures, and immediately to betake themselves to experiment. He has pursued this method on the subject of magnetism with wonderful ardour, and with equal genius and success; for Dr. Gilbert was possessed both of great ingenuity, and a mind fitted for general views of things. The work contains a prodigious number and variety of observations and experiments, collected with sagacity from the writings of others, and instituted by himself with considerable expence and labour. It would indeed be a miracle if all Dr. Gilbert s general inferences were just, or all his experiments accurate. It was untrodden ground. But, on the whole, this performance contains more real information than any writing of the age in which he lived, and is scarcely exceeded by any that has appeared since. We may hold it with justice as the first fruits of the Baconian or experimental philosophy.

This work of Dr. Gilbert's relates chiefly to the loadstone, and what we call magnets, that is, pieces of steel which have acquired properties similar to those of the loadstone. But he extends the term *magnetism*, and the epithet *magnetic*, to all bodies which are affected by loadstones and magnets in a manner similar to that in which they affect each other. In the course of his investigation, indeed, he finds that these bodies are only such as contain iron in some state or other: and in proving this limitation, he mentions a great variety of phenomena which have a considerable resemblance to those which he allows to be magnetical, namely, those which he called *electrical*, because they were produced in the same

way that amber is made to attract and repel light bodies. He marks with care the distinctions between these and the characteristic phenomena of magnets. He seems to have known, that all bodies may be rendered electrical, while ferrugineous substances alone can be made magnetical.

240. It is not saying too much of this work of Dr. Gilbert's to affirm, that it contains almost every thing that we know about magnetism. His unwearied diligence in searching every writing on the subject, and in getting information from navigators, and his incessant occupation in experiments, have left very few facts unknown to him. We meet with many things in the writings of posterior inquirers, some of them of high reputation, and of the present day, which are published and received as notable discoveries, but are contained in the rich collection of Dr. Gilbert. Dr. Gilbert's book, although one of those which does the highest honour to our country, is less known in Britain than on the Continent. Indeed we know but of two British editions of it, which are both in Latin; and we have seen five editions published in Germany and Holland before 1628. We earnestly recommend it to the perusal of the curious reader. He will find more facts in it than in the two large folios of Scarella.

241. In mechanical philosophy, a phenomenon is not to be considered as explained, unless we can shew that it is the certain result of the laws of motion applied to matter. It is in this way that the general propositions in physical astronomy, in the theory of machines, in hydraulics, &c. are demonstrated. But the phenomena called *magnetical* have not as yet obtained such an explanation. We do not see their immediate cause, nor can we say with confidence that they are the effects of any particular kind of matter, acting on the bodies either by impulsion or pressure.

All that can be done here is to class the phenomena in the most distinct manner, according to their generality. In this we obtain a two-fold advantage. We may take it for granted that the most general phenomenon is the nearest allied to

the general cause. But, farther, we obtain by this method a true theory of all the subordinate phenomena. For a just theory is only the pointing out the general fact of which the phenomenon under consideration is a particular instance. Beginning therefore with the phenomenon which comprehends all the particular cases, we explain those cases in shewing *in what manner* they are included in the general phenomenon, and thus we shall be able to predict what will be the result of putting the body under consideration into any particular situation. And perhaps we may find, in them all, coincidences which will enable us to shew that they are all modifications of a fact still more general. If we gain this point, we shall have established a complete theory of them, having discovered the general fact in which they are all comprehended. Should we for ever remain ignorant of the cause of this general fact, we have nevertheless rendered this a complete branch of mechanical theory. Nay, we may perhaps discover such circumstances of resemblance between this general fact and others, with which we are better acquainted, that we shall, with great probability at least, be able to assign the cause of the general fact itself, by shewing the law of which it is a particular instance.

We shall attempt this method on the present occasion.

242. The leading facts in magnetism are the two following:

1. If any oblong piece of iron, such as a bar, rod, or wire, be so fitted, that it can assume any direction, it will arrange itself in a certain determinate direction with respect to the axis of the earth. Thus, if, in any part of Britain, an iron or steel wire be thrust through a piece of cork, as represented in Plate III. fig. 1. so as that the whole may swim level in water, and if it be laid in the water nearly north-west and south-east, it will slowly change its position, and finally settle in a direction, making an angle of about 25 degrees with the meridian.

This experiment, which we owe to Dr. Gilbert (see B. I. ch. 11.), is delicate, and requires attention to many circumstances. The force with which the iron tends toward this final position is extremely weak, and will be balanced by very minute and otherwise insensible resistances; but we have never found it fail when executed as here directed. An iron wire of the size of an ordinary quill, and about eight or ten inches long, is very fit for the purpose. It should be thrust through the cork at right angles to its axis; and so adjusted, by repeated trials, as to swim level or parallel to the horizon. The experiment must also be made at a great distance from all iron; therefore in a bason of some other metal or earthen ware. It may sometimes require a very long while before the motion begin; and if the wire has been placed at right angles to the direction which we have mentioned as final, it will never change its position; therefore we have directed it to be laid in a direction not too remote, yet very sensibly different from the final direction.

But this is not the true position affected by the iron rod. If it be thrust through a piece of wood or cork perfectly spherical, in such a manner that it passes through its centre, and if the centre of gravity coincide with this centre, and the whole be of such weight as to remain in any part of the water, without either ascending or descending, then it will finally settle in a plane inclined to the meridian about 25°, and the north end will be depressed about 73° below the horizon.

All this is equivalent with saying, that if any oblong piece of iron or steel be very nicely poised on its centre of gravity, and at perfect liberty to turn round that centre in every direction, it will finally take the position now mentioned.

We have farther to observe with regard to this experiment, that it is indifferent which end of the rod be placed toward the north in the begining of the experiment. That

end will finally settle toward the north: and if the experiment be repeated with the same rod, but with the other end north, it will finally settle in this new attitude. It is, however, not always that we find pieces of iron thus perfectly indifferent. Very frequently one end affects the northerly position, and we cannot make the other end assume its place: the causes of this difference will be clearly seen by and bye.

243. The position thus affected by a rod of iron is called by Dr. Gilbert the MAGNETICAL POSITION or DIRECTION. It is not the same, nor parallel, in all parts of the earth, as will be more particularly noticed afterwards.

244.—2. The other leading fact is this: When a piece of iron, lying in the *magnetical* position, or nearly so, and at perfect liberty to move in every direction, is approached by another oblong piece of iron, held nearly in the same position, it is attracted by it: that is, the moveable piece of iron will gradually approach to the one that is presented to it, and will at last come into contact with it, and may then be slowly drawn along by it.

This phenomenon, although not so delicate as the former, is still very nice, because the attraction is so weak that it is balanced by almost insensible obstructions. But the experiment will scarcely fail if conducted as follows: Let a strong iron wire be made to float on water by means of a piece of cork, in the manner already described, having one end under water. See Plate III. fig. 1. B.

When it is nearly in the magnetical position, bring the end of a pretty big iron rod, such as the point of a new poker, within a quarter of an inch of its southern end (holding the poker in a position not very different from the magnetical position), and hold it there for some time, not exactly southward from it, but a little to one side. The floating iron will be observed to turn towards it with an accelerated motion: will touch it, and may then be drawn by it through the water in any direction. We shall have the same result

by approaching the northern extremity of the floating iron with the upper end of the poker.

The same phenomena may be observed by suspending the first piece of iron by its middle by a long and slender hair or thread. The suspension must be long, otherwise the stiffness of the hair or thread may be sufficient for balancing the very small force with which the pieces of iron tend toward each other. The phenomenon may also be observed in a piece of iron which turns freely on a fine point, like the needle of the mariner's compass.

In this, as in the former experiment, the ends of the pieces of iron are observed, in general, to be indifferent; that is, either end of the one will attract either end of the other. It often happens, however, that the ends are not thus indifferent, and that the end of the moveable piece of iron, instead of approaching the other, will be observed to recede from it, and appear to avoid it. We shall soon learn the cause of this difference in the states of iron.

245. It is scarcely necessary to remark, that we must infer from these experiments, that the action is mutual between the two pieces of iron. Either of them may be the moveable piece which approaches the other, manifesting the attraction of that other. This reciprocity of action will be abundantly verified and explained in its proper place.

246. These two facts were long thought to be peculiar to loadstones and artificial magnets, that is, pieces of iron which have acquired this property by certain treatment with loadstones; but they were discovered by Dr. Gilbert to be inherent in all iron in its metallic state; and were thought by him to be necessary consequences of a general principle in the constitution of this globe. These phenomena are indeed much more conspicuous in loadstones and magnets; and it is therefore with such that experiments are best made for learning their various modifications.

247. But there is another circumstance, besides the degree of vivacity, in which the magnetism of common iron and

steel remarkably differs from that of a loadstone or magnet. When a loadstone or magnet is so supported as to be at liberty to take any position, it arranges itself in the magnetical direction, and *one determined end* of it settles in the northern quarter; and if it be placed so that the other end is in that situation, it does not remain there, but gradually turns round, and, after a few oscillations, the *same end* ultimately settles in the north. This is distinctly seen in the needle of the mariner's compass, which is just a small magnet prepared in the same way with all other magnets. The several ends of loadstones or magnets are thus permanently the north or the south ends; whereas we said that either end of a piece of common iron being turned to the northern quarter, it finally settles there.

It is this circumstance which has rendered magnetism so precious a discovery to mankind, by furnishing us with the compass, an instrument by which we learn the different quarters of the horizon, and which thus tells the direction of a ship's course through the pathless ocean; and also shews us the directions of the veins and workings in the deepest mines. It was natural therefore to call those the north and south ends of the mariner's needle, or of a loadstone or magnet. Dr. Gilbert called them the POLES of the loadstone or magnet. He had found it convenient for the proposed train of his experiments to form his loadstones into spheres, which he called TERRELLÆ, from their resemblance to this globe; in which case the north and south ends of his loadstones were the poles of the terrellæ. He therefore gave the name *pole* to that part of any loadstone or magnet which thus turned to the north or south. The denomination was adopted by all subsequent writers, and now makes a term in the language of magnetism.

248. Also, when we approach either end of a piece of iron A to either end of another B, these ends mutually attract; or if either end of a magnet A be brought near either end of a piece of common iron, they mutually attract each other.

But if we bring that end of a magnet A which turns to the north near to the *similar* end of another magnet B, these ends will not attract each other, but, on the contrary, will repel. If the two magnets are made to float on pieces of wood, and have their north poles fronting each other, the magnets will retire from each other; and in doing so, they generally turn round their axes, till the north pole of one front the south pole of the other, and then they run together. This is a very notable distinction between the magnetism of magnets and that of common iron; and whenever we see a piece of iron shew this permanent distinction of its ends, we must consider it as a magnet, and conclude that it has met with some peculiar treatment.

It is not, however, strictly true, that the poles of loadstones or magnets are so fixed in particular parts of their substance, nor that the poles of the same name so constantly repel each other; for if a small or weak magnet A have its pole brought near the similar pole of a large or strong magnet B, they are often found to attract when almost touching, although at more considerable distances they repel each other. But this is not an exception to the general proposition; for when the north pole of A is thus attracted by the north pole of B, it will be found, by other trials, to have all the qualities of a south pole, while thus in the neighbourhood of the north pole of B.

249. The magnetic properties and phenomena are conveniently distinguished into those of FORCE and of POLARITY. Those of the first class only were known to the ancients, and even of them their knowledge was extremely scanty and imperfect. They may all be classed under the following general propositions.

250.—1. The similar poles of two magnets repel each other with a force decreasing as the distances increase.

2. The dissimilar poles of two magnets attract each other with a force decreasing as the distances increase.

3. Magnets arrange themselves in a certain determinate position with respect to each other.

251. The first object of research in our farther examination of these properties is the relation which is observed to obtain between the distances of the acting poles and their force of action. This has accordingly occupied much attention of the philosophers, and numberless experiments have been made in order to ascertain the law of variation, both of the attraction and the repulsion. It is obvious, from the nature of the thing, that the determination is very difficult, and the investigation very complicated. We can only observe the simultaneous motion of the whole magnet; yet we know that there are four separate actions co-existing and contributing in different directions, and with different forces, to the sensible effect. The force which we measure, in any way whatever, is compounded of four different forces, which we cannot separate and measure apart; for the north pole of A repels the north pole of B, and attracts its south pole, while the south pole of A exerts the opposite forces on the same poles of B. The attraction which we observe is the excess of two unequal attractions above two unequal repulsions. The same might be said of an observed repulsion. Nay, the matter is incomparably more complicated than this; because, for any thing that we know, *every* particle of A acts on *every* particle of B, and is acted on by it; and the intensity of those actions may be different at the same distances, and is certainly different when the distances are so. Thus there is a combination of an unknown number of actions, each of which is unknown individually, both in direction and intensity. The precise determination is therefore, in all probability, impossible. By precise determination, we mean the law of mutual action between two magnetic particles, or that precise function of the distance which defines the intensity of the force; so that measuring the distance of the acting particles on the axis of a curve,

the ordinates of the curve may have the proportions of the attractions and repulsions.

It is almost needless to attempt any deduction of the law of variation from the numerous experiments which have been published by different philosophers. An ample collection of them may be seen in Scarella's treatise. Mr. Muschenbrock has made a prodigious number; but all are so anomalous, and exhibit such different laws of diminution by an increase of distance, that we may be certain that the experiments have been injudicious. Attention has not been paid to the proper objects. Magnets of most improper shapes have been employed, and of most diffuse polarity. No notice has been taken of a circumstance which, one should think, ought to have occupied the chief attention; namely, the joint action of four poles, of which the experiment exhibits only the complex result. A very slight reflection might have made the inquirer perceive, that the attractions or repulsions are not the most proper phenomena for declaring the precise law of variation; because what we observe is only the excess of a small difference of attractions and repulsions above another small difference. Mr. Hawksbee and Dr. Brook Taylor employed a much better method, by observing the deviations from the meridian which a magnet occasioned in a compass needle at different distances. This is occasioned by the difference of the two sums of the same forces; and this difference may be made a hundred times greater than the other. But they employed magnets of most improper shapes *.

* The true law of Magnetic action was deduced from these experiments so long ago as 1750, by Mr. Michell, in his work entitled "A Treatise of Artificial Magnets."

"There have been, "he observes," some who have imagined that the decrease of the magnetic attraction and repulsion is inversely as the cubes of the distances; others as the squares, and others that it follows no certain ratio at all, but that it is much quicker at great distances than at small ones, and that it is different in different stones; among these last, is Dr. Brook Taylor and Muschenbroek, who seem to have been pretty accurate in their experiments. (See *Phil. Trans.* No. 368 and 390.) The conclusions of these Gentlemen were

252. We must except from this criticism the experiments of Mr. Lambert, recorded in the Memoirs of the Academy of Berlin for 1756, published in 1758. This most sagacious philosopher (for he highly merits that name) placed a mariner's needle at various distances from a magnet, in the direction of its axis, and observed the declination from the magnetic meridian produced by the magnet, and the obliquity of the magnet to the axis of the needle. Thus, was the action of the magnet set in opposition and equilibrium with the natural polarity of the needle. But the difficulty was to discover in what proportion each of those forces was changed by their obliquity of action on this little lever. No man excelled Mr. Lambert in address in devising methods of mathematical investigation. He observed, that when the obliquity of the magnet to the axis of the needle was 30°, it caused it to decline 15°. When the obliquity was 75°, the distance being the same, it declined 30°. Call the obliquity o, and the declination d, and let f be that function of the angle which is proportionable to the action. Also let p be the natural polarity of the needle, and m the force of the magnet. It is evident that

$$p \times f, 15 = m \times f, 30$$

And $p : m = f, 30 : f, 15$; for the same reason

$$p : m = f, 75 : f, 30$$

Therefore $f, 15 : f\,30 = f, 30 : f, 75°$.

But it is well known that

$$\text{Sine } 15 : \text{Sine } 30 = \text{Sine } 30 : \text{Sine } 75.$$

Hence Mr. Lambert was led to conjecture, that the sine was that function of the angle which was proportional to the action of magnetism on a lever. But one experiment was insufficient for determining this point. He made a similar comparison of several other obliquities and declinations with

drawn from their experiments, without their being aware of the third property of magnets just mentioned, which, if they had made proper allowances for, together with the increase and diminution of power in the magnets they tried their experiments with, all the irregularities they complained of, (as far as appears from their relations of them) might very well be accounted for, and *the whole of their experiments coincide with the squares of the distances inversely.*" P. 19, Note.—Ed.

the same distances of the magnet, and also with other distances; and he put it past all dispute, that his conjecture was just.

Had Mr. Lambert's experiments terminated here, it must be granted that he has made a notable discovery in the theory of the intimate nature of magnetism. It completely refutes all the theories which pretend to explain the action of a magnet by the impulsion of a stream of fluid, or by pressure arising from the motion of such a stream; for in this case the pressure on the needle must have diminished in the duplicate ratio of the sine. The directive power with the angle 90 must be 4 times greater than with the angle 30°; whereas it was observed to be only twice as great. Magnetism does not act therefore by the impulsion or pressure of a stream of fluid, but in the manner of a simple incitement, as we conceive attraction or repulsion to act.

Having ascertained the effect of obliquity, Mr. Lambert proceeded to examine the effect of distance; and, by a most ingenious analysis of his observations, he discovered, that if we represent the force of the magnet by f, and the distance of the nearest pole of the magnet from the centre of the needle by δ, and if a be a constant quantity, nearly equal to two-thirds of the length of the needle, we have f proportional to $\frac{1}{\delta - a^2}$.

Mr. Lambert found this hold with very great exactness with magnets ten times larger, and needles twice as short. But he acknowledges, that it gives a very singular result, as if the action of a magnet were exerted from a centre beyond itself. He attributes this to its true cause, the still great complication of the result, arising from the action of the remote pole of the magnet. He therefore takes another method of examination, which we shall understand by and by, when we consider the *directive* power of a magnet. We have mentioned this imperfect attempt chiefly on account of the unquestionable manner in which he has ascertained the effect of obliquity, and the importance of this determination.

We have attempted this investigation in a very simple manner. We got some magnets made, consisting of two balls connected by a slender rod. By a very particular mode of impregnation, we gave them a pretty good magnetism; and the force of each pole seemed to reside almost in the centre of the ball. This was our object in giving them this shape. It reduced the examination both of the attractive and of the directive power to a very easy computation. The result was, that the force of each pole varied in the inverse duplicate ratio of the distance. The error of this hypothesis in no case amounted to $\frac{1}{75}$th of the whole. In computing for the phenomena of the directive power, the irregularities and deviations from this ratio were much smaller.

The previous knowledge of this function would greatly expedite and facilitate our farther investigation: but we must content ourselves with a very imperfect approximation, and with arriving at the desired determination by degrees, and by a very circuitous route.

253. It is a matter of experience, that when two magnets are taken, each of which is as nearly equal as possible in the strength of both poles, then, if they are placed with their axes in one straight line, and the north pole of one fronting the south pole of the other, they attract each other with a force which diminishes as the distance increases; and this variation of force is regular, that is, without any sudden changes of intensity, till it becomes insensible. No instance has occurred of its breaking suddenly off when of any sensible force, but it appears to diminish continually like gravity. No instance occurs in which attraction is changed into repulsion.

But it is, moreover, to be particularly remarked, that, having made this observation with the north pole of A fronting the south pole of B, if the experiment be repeated with the south pole of A fronting the north pole of B, the results will be precisely the same. And, lastly, it is a matter of un-

excepted experience, that the sensible action of A on B, measured by the force which is necessary for preventing the farther approach of B, is precisely equal to the action of B on A. This is the case, however unequal the force of the two magnets may be; that is, although A may support ten pounds of iron, and B only ten ounces.

Now, the simplest view we can take of this experiment is, by supposing the whole action of one end or pole of a magnet to be exerted at one point of it. This will give us four actions of A on B, accompanied by as many equal and opposite actions of B on A. It is plain that we may content ourselves with the investigation of one only of these sets of actions.

What we observe is the excess of the attractions of the poles of A for the dissimilar poles of B above the repulsions of the same poles of A for the similar poles of B. At all distances there is such an excess. The sum of the attractions exceeds the sum of the repulsions competent to every distance.

Now this will really happen, if we suppose that the poles of a magnet are of equal strength, and that, however these different magnets differ in strength, they have the same law of diminution by an increase of distance. The first circumstance is a very possible thing, and the last is demonstrated by the observed equality of action and reaction. Every thing will now appear very plain, by representing (Plate III.) the intensities of attraction and repulsion by the ordinates of a curve, of which the abscissæ represent the distances of the acting poles.

Therefore let A and B (Plate III. fig. 2.) represent the two magnets, placed with their four poles S, N, *s*, *n*, in a straight line. In the straight line O *q* take O *m*, O *p*, O *n*, O *q*, respectively equal to N *s*, N *n*, S *s*, S *n*; and let MPNQ be a curve line, having O *q* for its axis and assymptote; and let the curve, in every part, be convex towards its axis. Then draw the ordinates *m* M, *p* P, *n* N, *q* Q, to the curve.

These ordinates will represent the intensities of the forces exerted between the poles of the magnets, in such a manner as to fulfil all the conditions that are really observed: For m M represents the attraction of the north pole N of the magnet, A for the south pole s of the magnet B; p P represents the repulsion of N for n; n N represents the repulsion of S for s; and q Q represents the attraction of S for n. The distance between m and n, or between p and q, is equal to the length of the magnet A, and $m\,p$, or $n\,q$, is equal to that of B. M m, P p, and N n, Q q, are pairs of equidistant ordinates. It surely requires only the inspection of the figure to see that, in whatever situation along the axis we place those pairs of equidistant ordinates, the sum of M m and Q q will always exceed the sum of P p, and N n; that is, the sum of the attractions will always exceed that of the repulsions. This will not be the case if the curve, whose ordinates are proportional to the forces, have a point Z of contrary flexure, as is represented by the dotted curve P′ZQ′. For this curve, having O q for its assymptote (in order to correspond with forces which diminish continually by an increase of distance, but do not abruptly cease) must have its convexity turned toward this assymptote in the remote parts. But there will be an arch MPZ between Z and O, which is concave toward the assymptote. In which case, it is possible that M m + Q q shall be less than P p + N n; and then the repulsions will exceed the attractions; which is contrary to the whole train of observation.

It may be thought, that if the repulsion exerted between two particles be always less than the attraction at the same distance, the phenomena will be accounted for, although the law of action be not represented by such a curve as has been assumed. Undoubtedly they will, while the dissimilar poles front each other: But the results of such a supposition will not agree with the phenomena while the similar poles front each other. For it is an uncontradicted fact, that when two fine hard magnets, whose poles are near-

ly or exactly of equal vigour, have their similar poles fronting each other, the repulsions fall very little short of the attractions at the same distances when their position is changed: When the distances are considerable, scarcely any difference can be observed in the beginning of the experiment. The differences, also, which are observed at smaller distances, are observed to augment by continuing the magnets in their places without changing their distances; and therefore seem to arise from some change produced by each on the magnetism of the other. And, accordingly, if we invert one of the magnets, we shall find that the attractions have been diminished as much as the repulsions. Now, the consequences of magnetic repulsion, being always weaker than attraction, would be the reverse of this. The differences would appear most remarkable in the greater distances, and magnets might be found which repel at small distances, and attract at greater distances; which is contrary to all observation.

From all this it follows, with sufficient evidence for our present purpose, that the function of the distance which expresses the law of magnetic action must be represented by the ordinates of a curve of the hyperbolic kind, referred to its assymptote as an axis; and therefore always convex toward this axis. We think it also sufficiently clear, that the consequences which we have deduced from the simple supposition of four acting points, instead of the combined action of every particle, may be adopted with safety. For they would be just, if there were only those four particles; they would be just with respect to another four particles—therefore they would be just when these are joined; and so on of any number. Therefore the curve, whose ordinates express the mean action of each pole, as if exerted by its centre of effort, will have the same general form: It will be convex toward its assymptotic axis.

It will greatly aid our conceptions of the combined actions of the four magnetic poles, if we notice some of the

primary properties of a curve of this kind, limited by no other condition.

254. Draw the chords MQ, PN, MP, NQ. Bisect them in B, D, E, F, and join EF. Draw the ordinates E e F f, and BD b (cutting EF in C). Draw P u parallel to the axis, cutting E e in ε. Draw also Q i parallel to the axis, cutting F f in φ. Also draw FHL parallel to the axis, and P o t parallel to QN; and draw P L l, and P e x, cutting M m in l and x.

Let each ordinate be represented by the letter at its intersection with the axis. Thus, the ordinates M m and Q q may be represented by m and q, &c.

Because MP is bisected in E, M t is double of E ε; M l is double of EL; M x is double of E e. Also, because P t is parallel to QN, and P u to Q i, we have $t\,u = \mathrm{N}\,i$. From these premises, it is easy to perceive, that,

1. $\mathrm{B}\,b = \frac{m+q}{2}$.
2. $\mathrm{D}\,b = \frac{p+n}{2}$.
3. $\mathrm{BD} = \frac{\overline{m+q} - \overline{p+n}}{2}$.
4. $\mathrm{M}\,u = m - p$.
5. $u\,t = n - q$.
6. $\mathrm{M}\,t = \overline{m-p} - \overline{n-q}$.
7. $\mathrm{E}\,e = \frac{m+p}{2}$.
8. $\mathrm{F}\,f = \frac{n+q}{2}$.
9. $\mathrm{M}\,l = \overline{m+p} - \overline{n+q}$.
10. $\mathrm{EL} = \frac{\overline{m+p} - \overline{n+q}}{2}$.
11. $\mathrm{CD} = \frac{\overline{m+q} - \overline{p+n}}{4}$.
12. $\mathrm{CH} = \frac{\overline{m+p} - \overline{n+q}}{4}$.

These combinations will suggest to the attentive reader the explanation of many modifications of the combined action of the four poles of two magnets. They are all comprehended in one proposition, which it will be convenient to render familiar to the thought; namely, if two pairs of equidistant ordinates be taken, the sum of the two extremes exceeds that of the intermediate ones. $m + q$ is greater than $p + n$. Also, the difference between the pair nearest to O exceeds the difference between the remote pair.

Now, conceiving these ordinates to represent the mutual actions of the magnetic poles, we see that their tendency to or from each other, or their sensible attractions or repulsions, are expressed by $\overline{m + q} - \overline{n + p}$; that is, by the excess of the sum of the actions of the nearest and most remote poles above the sum of the actions of the intermediately distant poles. It will also be frequently convenient to consider this tendency as represented by $\overline{m - p} - \overline{n - q}$; that is, by the excess of the difference of the actions of the nearest pole of A on the two poles of B, above the difference of the actions of its remote pole on the same poles of B.

Let us now consider some of the chief modifications of these actions.

255.—1. Let the dissimilar poles front each other. It is plain that $m + q$ represent attractions, and that $p + n$ represent repulsions. Also $m + q$ is greater than $p + n$. Therefore the magnets will attract each other. This attraction is also represented by $\overline{m - p} - \overline{n - q}$.

Now $\overline{m + q} - \overline{p + n}$ is evidently equal to M t, or to twice E o, or to twice BD, or to four times CD.

This action will be increased,

1. By increasing the strength of either of the magnets. The action of the magnets is the combined action of each acting particle of the one on each acting particle of the other; and it is mutual. Therefore all the ordinates will increase

in the ratio of the strength of each magnet, and their sums and differences will increase in the same ratio.

2. By diminishing the distance between the magnets. For this brings all the ordinates nearer to O, while their distances $m\,p$, $p\,n$, $n\,q$, remain as before. In this case it is plain, that M u, the difference of M m and P p, will increase faster than $t\,u$ or N i, the difference between N n and Q q. Therefore M t will increase; that is, the attraction will increase.

3. By increasing the length of A, while the distance between them remains the same. For O m remaining the same, as also $m\,p$ and $n\,q$, while $n\,q$ is only removed farther from $m\,p$, it is plain M u remains the same, and that N i and $t\,u$ are diminished; therefore M t must increase, or the attraction must increase.

4. By increasing the length of B, the distance between them remaining the same. For this increases $m\,p$ and $n\,q$; and consequently increases M u and $t\,u$. But M u increases more than $t\,u$; and therefore M t is increased, and the attraction or tendency is increased.

All these consequences of our original supposition, that the magnetic action may be represented by the ordinates of a curve every where convex to an assymptotic axis, are strictly conformable to observation.

256. If we place the magnets with their similar poles fronting each other, it is evident that the ordinates which expressed attractions in the former case, will now express repulsions; and that the forces with which the magnets now repel each other, are equal to those with which they attracted when at the same distances. When the experiments are made with good loadstones, or very fine magnets, tempered extremely hard, and having the energy of their poles sensibly residing in a small space very near the extremities, the results are also very nearly conformable to this mathematical theory; but there is generally a weaker action. The magnets seldom repel as strongly as they attract at the same distance; at least when these distances are small. If one

or both of the magnets is soft, or if one of them be much more vigorous than the other, there are observed much greater deviations from this theory. The repulsions are *considerably* weaker than the attractions at the same distance, and the law of variation becomes extremely different. When placed at very considerable distances, they repel. As the magnet B is brought nearer to A, the repulsion increases, agreeably to the theory, but not so fast. Bringing them still nearer, the repulsion ceases to increase, then gradually diminishes, and frequently vanishes altogether, before the magnets are in contact; and when brought still nearer, it is changed into attraction.

257. But more careful observation shews, that this anomaly does not invalidate the theory. It is found that the vigour of the magnets is permanently changed by this process. The magnets act on each other in such a way as to weaken each other's magnetism. Nay, it frequently happens, that the weaker or the softer of the two has had its magnetism changed, and that the pole nearest to the other has changed its nature. While they are lying in contact, or at such a distance that they attract, although their similar poles front each other, it is found that the pole of one of them is really changed; although it may sometimes recover its former species again, but never so vigorously as when the other magnet is removed. In short, it is observed, that the magnetism is diminished in all experiments in which the magnets repel each other, and that it is improved in all experiments in which they attract.

We have hitherto supposed the magnets placed with their axes in one straight line. If they are differently placed, we cannot ascertain by this single circumstance of the law of magnetic action, whether they will attract or repel—we must know somewhat more of the variation of force by a change of distance.

258. If the magnet B be not at liberty to approach toward A, or recede from it, but be so supported at its centre B

that it can turn round it, it is very plain that it will retain the position in which it is drawn in the figure. For its south pole *s* being more attracted by N than it is repelled by S, is on the whole, attracted by the magnet A; and, by this attraction, it would vibrate like a pendulum that is supported at the centre B. In like manner, its north pole *n* is more repelled by N than it is attracted by S, and is, on the whole, repelled. The part B *n* would therefore also vibrate like a pendulum round B. Thus each half of it is urged into the very position which it now has; and if this position be deranged a little, the attraction of *s* B toward A, and the repulsion of *n* B from it, would impel it toward the position *s* B *n*.

This will be very evident, if we put the magnet B into the position *s'*B*n'*, at right angles to the line AB. The pole *s'* and the pole *n'* are urged in opposite, and therefore conspiring, directions, with equal forces, very nearly at right angles to *n' s'*, if the magnet B be small. In any oblique position, the forces will be somewhat unequal, and account must be had of the obliquity of the actions, in order to know the precise rotative momentum of each.

Dr. Gilbert has given to this modification of the action of A on B, the name of VIS DISPONENS; which we may translate by DIRECTIVE POWER or FORCE. Also, that modification of the tendency of B to or from A is called by him the VERTICITAS of B. We might call it the VERTICITY of B; but we think that the name POLARITY is sufficiently expressive of the phenomenon; and as it has come into general use, we shall abide by it.

259. It is not so easy to give a general, and at the same time precise, measure of the directive power of A and polarity of B. The magnet B must be considered as a lever; and then the force tending to bring it into its ultimate position *n s* depends both on the distance of its poles from N and S, and also on the angle which the axis of B makes

with the line AB. When the axis of B coincides with AB, the force acting on its poles, tending to *keep* them in that situation, is evidently $\overline{m+p} - \overline{n+q}$, and therefore may be represented by M l (in Plate III. fig. 2.), or by twice EL, or by four times CH. If B has the position n' B s', perpendicular to AB, let the ordinates E e and F f cut the curve in I and K: and draw KL parallel to the axis (our figure causes this line almost to coincide with FL, and in all important cases it will be nearly the same). In this case IL will express one half of this force. Either of these estimations of this modification of the mutual action of the magnets, will be sufficient for the objects we have in view.

260. The directive power of A, and the polarity of B, are increased,

1. By increasing the strength of one or both of the magnets. This is evident,

2. By diminishing the distance of the magnets. For this, by increasing the sum of M m and P p more than the sum of N n and Q q, must increase EL or M l.

3. By increasing the length of A. For this, by removing n and q farther from m and p, must depress the points I. and l, and increase EL, or IL, or M l.

4. By diminishing the length of B, while the distance N s between the magnets remains the same. For this, by bringing p and q nearer to m and n, must increase M m + P p more than N n + Q q. Or, by bringing E e and F f nearer to M m and N n, it must increase EL and M l.

If the distance N n, between the pole of A and the remote pole of B remain the same, the directive force of A, and polarity of B, are diminished by diminishing the length of B, as is easily seen from what has been just now said. It is also diminished, but in a very small degree, by diminishing the length of B, when the distance between the centres of A and B remain the same. For, in this case, the ordinates I e and K f retain their places; but the points m and p approach to e; and this brings the intersection E of the

ordinate and chord nearer to I, and diminishes EL, because the point L is not so much depressed by the approach of F to K as E is depressed.

261. But in all cases, the *ratio* of the directive power of A to its attractive force, or of the polarity of B to its tendency to A, is increased by diminishing the length of B. For it is plain, that by diminishing *m p* and *n q*, while I *e* and K *f* keep their places, the point *o* is raised, and the point L is depressed; and therefore the ratio of EL to E *o*, or of M *l*, to M *t*, is increased. We even see that, by diminishing the length of B continually and without end, the ratio of M *l* to M *t* may be made to exceed any ratio that can be assigned.

262. Now, since diminishing the length of B increases the ratio of the directive power of A to its attractive power, while increasing the length of A increases both, and also increases the ratio of EL to E *o* (as is very easily seen), and since this increase may be as great as we please, it necessarily follows, that if the same very small magnet B be placed at such distances from a large and strong magnet A, and from a smaller and less vigorous one C, as to have equal polarities to both, its tendency to A will be less than its tendency to C It may even be less in any ratio we please, by sufficiently diminishing the length of B.

Dr. Gilbert observed this; and he expresses his observation by saying, that the directive power extends to greater distances than the attracting power. We must just conclude, that the last becomes *insensible* at smaller distances than the first. This will be found a very important observation. It may be of use to keep in mind, that the directive power of a magnet A on another magnet B, is the difference of the *sums* of the actions of each pole of A on both poles of B; and the attractive power of A for another magnet B, is the difference of the *differences* of these actions.

It may be also remarked just now, that the directive force of A always exceeds its attractive force by the quantity

$2\ (p-q)$. For their difference may be expressed by $t\ l$, which is equal to twice o L. Now $\varepsilon\ e$ is equal to P p, or to p; and ε L is equal to P p — F f, or to P p — Q q — F φ, or to P p — Q q — $o\ \varepsilon$. Therefore o L = P p — Q q, and $t\ l = 2$ (P p — Q q), $= 2\ (p-q)$.

By inspecting this figure with attention, we obtain indications of many interesting particulars. If the lengths of the magnets A and B are the same, the point n in the axis of the curve will coincide with p. As the length of A increases, the part $n\ q$ is removed farther from the part $m\ p$. The line P t becomes less inclined to the axis, and is ultimately parallel to it, when n is infinitely remote. At this time L falls on e; so that the ultimate ratio of the attraction to the polarity is that of E ε to E e, when the magnet A is infinitely long. It is then the ratio of the difference of the actions of the nearest pole of A on the two poles of B to the sum of these actions. Hence it follows, that when A is very great and B very small, the polarity of B is vastly greater than its tendency to A. It may have a great polarity when its tendency is insensible.

The ratio of the polarity to the attraction also increases by increasing the distance of the magnets while their dimensions continue the same. This will appear, by remarking that the chords MP and NQ must intersect in some point w; and that when the four points m, p, n, and q, move off from O, keeping the same distances from each other, E o will diminish faster than EL, and the ratio of EL to E o will continually increase.

Therefore when a small magnet B is placed at such a distance from a great magnet A, and from a smaller one C, as to have equal polarity to both, its tendency to C will exceed its tendency to A. For the polarities being equal, it must be farther from the great magnet; in which case the ratio of its polarity to its attraction is increased.

And this will also obtain if the magnets differ also in strength. For, to have equal polarities, B must be still farther from the great and powerful magnet.

For all these reasons, a large and powerful magnet may exert a strong directive power, while its attractive power is insensible.

263. We have hitherto supposed the magnet B to be placed in the direction of the axis of A, and only at liberty to turnround its centre B. But let its centre be placed on the centre of A, as in Plate III. fig. 3. it must evidently take a position which may be called sub-contrary to that of A, the north pole of B turning toward the south pole of A, and its south pole turning toward the north pole of A.

The same thing must happen when the centre of B is placed in B, any where in the line AE perpendicular to NS. S attracts *n* with a force *n b*, while N repels *n* with a force *n c*, somewhat smaller than *n b*. These two compose the force *n d*. In like manner, the two forces *s e* and *s f*, exerted by N and S on the pole *s*, compose the force *s g*. Now if the axis of the magnet B be parallel to NS, but the poles in a contrary position, and if each magnet be equally vigorous in both poles, the magnet B will retain this position; because the forces *n b* and *s e* are equal, as also the forces *n c* and *s f*. These must compose two forces *n d* and *s g*, which are equal, and equally inclined to *n s*; and they will therefore be in equilibrio on this lever.

Let us now place the centre of the small magnet in C, neither in the axis of the other, nor in the perpendicular AE. Let its north pole *n* point toward the centre of A. It cannot remain in this position; for N repels *n* with a force *n c*, while S attracts it with a force *n b* (smaller than *n c*, because the distance is greater). These two compose a force *n d* considerably different from the direction C *n* of its axis. In like manner, the south pole *s* of the small magnet is acted on by two forces *s e* and *s f*, exerted by the two poles of A, which compose a force *s q* nearly equal and parallel to *n d*, but in a nearly opposite direction. It is plain that these forces must turn the small magnet round its centre C, and that it cannot rest but in a position nearly parallel to *n d* or

g. Its posititon is better represented by Plate III. fig. 4. with its south pole turned toward the north pole of the other magnet, and its north pole in the opposite direction.

What the precise position will be, depends on that function of the distance which is always proportional to the intensity of the action; on the force of each of the poles of A, and on the length of the magnet B. Nay, even when we know this function, the problem is still very intricate.

264. There are methods by which we may approximate to the function with success. If the magnet B be indefinitely small, so that we may consider the actions on its two poles as equal, the investigation is greatly simplified. For, in this case, each pole of the small magnet B (Plate III. fig. 5.) may be conceived as coinciding with its centre. Then, drawing NB, SB, and taking B *b* toward N, to represent the force with which N attracts the south pole of B, and taking B *c*, in SB produced, to represent the force with which S repels the same pole, the compound force acting on this pole is B *d*, the diagonal of a parallelogram B *b d c*. In like manner, we must take B *e*, in N *b* produced, and equal to B *b*, to represent the repulsion of N for the north pole of B, and B *f* equal to B *c*, to represent the attraction of S for this pole. The compound force will be B *g*, equal and opposite to B *d*. It follows evidently from this investigation, that the small magnet will not rest in any position but *d g*. In this supposition, therefore, of extreme minuteness of the magnet B, one of the parallelograms is sufficient. We may farther remark, that we have this approximation secure against any error arising from the supposition that all the action of each pole of B is exerted by one point. Although we suppose it diffused over a considerable portion of the magnet, still the extreme minuteness of the whole makes the action, even on its extreme points, very nearly equal.

265. Hence may be derived a construction for ascertaining the position of the needle, when the function *m* of the

distance is given, or for discovering this function by observation of the position of the needle.

Let NS (Plate III. fig. 5. n° 2.) meet the direction of the needle in K. Make BG = BN, and draw NF, GE, SH, perpendicular to BK. It is evident that Bb is to Bc or $b\,d$, as the sine of the angle HBS to the sine of KBN. Therefore, because BG and BN are equal, we have Bb : Bc = GE : NF.

Therefore	GE : NF = BS^m	: BN^m
But	SH : GE = BS	: BN
Therefore	SH : NF = BS^{m+1}	: BN^{m+1}
And	SK : NK = BS^{m+1}	: BN^{m+1}

If magnetic action be inversely as the distance, we have SK : NK = BS^2 : BN^2, and B is in the circumference of a circle which passes through S and N, and has BK for a tangent, as is plain by elementary geometry. If the action be inversely as the square of the distance, we have SK : NK = BS^3 : BN^3, and B is in the circumference of a curve of more difficult investigation. But, as in the circle, the sum of the angles BSN and BNS is a constant angle; so, in this curve, the sum of the cosines of those angles is a constant quantity. This suggests a very simple construction of the curve. Let it pass through the point T of the line AT, drawn from the centre of the magnet, perpendicular to its axis. Describe the semicircle SPQN, cutting ST and NT in P and Q. Then, in order to find the point where any line SB cuts the curve, let it cut the semicircle in p, and apply the the line N q = SP + NQ — S p, and produce it till it meet the line SB in B, which is a point in the curve; for it is evident that S p and N q are the cosines of BSN and BNS. We hope to give, by the help of a learned friend, the complete construction of curves for every value of m, in an Appendix to this article. It will form a new and curious class, arranged by the functions of the angles at N and S.

But, in the mean time, we have determined the position of an indefinitely small needle, in respect of a magnet of

which we may conceive the polar activity concentrated in two points; and we may, on the other hand, make use of the observed positions of such a needle and magnet for discovering the value of m. For, since $\frac{SK}{NK} = \frac{SB^{m+1}}{NB^{m-1}}$, it is plain that $m = \frac{\text{Log. SK : NK}}{\text{Log. SB : NB}} - 1$. Thus, in an observation which the writer of this article made on a very small needle, and a magnet having globular poles, and $8\frac{1}{8}$ inches between their centres, he found SB = 8, NB = $5\frac{1}{3}$, SK = 11,49, and NK = 3,37. This gives $m = 2{,}02$, which differs very little from 2. Finding it so very near the inverse duplicate ratio of the distance, a circle VUZ was described, the circumference of which is the locus of SB : BN = 8 : 5,333. When the centre of the needle was placed any where in the circumference of this circle, it scarcely deviated from the point K, except when so far removed from the magnet that its natural polarity prevailed over the directive power of the magnet, or so near its middle that the action of the cylindrical part became very sensible.

It is plain that the length of the needle must occasion some deviation from the magnetic direction, by destroying the perfect equality of action on its two poles. He therefore employed three needles of $\frac{1}{2}$, $\frac{1}{3}$, and $\frac{1}{4}$ of an inch in length; and by noticing the differences of direction, he inferred what would be the direction, if the forces on each pole were precisely equal. He had the pleasure of seeing that the deviation from the inverse duplicate ratio of the distances was scarcely perceptible.

Mr. Lambert's experiments on the directive power of the magnet, narrated in his second dissertation in the 22d volume of the Memoirs of the Academy of Berlin, are the most valuable of all that are on record; and the ingenious address with which they are conducted, and the inferences are drawn, would have done credit to Newton himself. We earnestly recommend the careful perusal of that Essay, as

the most instructive of any that we have read. The writer of this found himself obliged to repeat all his former experiments, mentioned above, in Mr. Lambert's manner, and with his precaution of keeping the needle in its natural position; a circumstance to which he had not sufficiently attended before. The new results were still more conformable to his conjecture as to the law of variation. Mr. Lambert closes his dissertation with an hypothesis, "that the force of each transverse element of a magnet is as its distance from the centre, and its action on a particle of another magnet is inversely as the square of the distance." On this supposition, he calculates the position of a very small needle, and draws three of the curves to which it should be the tangent. These are very exactly coincident with some that he observed. We tried this with several magnetic bars, and found it very conformable to observation in some magnets; but deviating so far in the case of other magnets, that we are convinced that there is no rule for the force of each transverse element of a magnet, and that the magnetism is differently disposed in different magnets. It was chiefly this which induced us to form the magnets employed in this research of two balls united by a slender rod. Lichtenberg, in his notes on Erxleben's Natural Philosophy, says, that there is a MS. of the celebrated Tobias Mayer in the library of the Academy of Gottingen, in which he assumes the hypothesis above mentioned, and gives a construction of the magnetic curves founded on it, making them a kind of catenaria. The interior curves do indeed resemble the catenaria, but the exterior are totally unlike. But there is no occasion for much argument to convince us, that the first part of this hypothesis is not only gratuitous, but unwarranted by any general phenomena. We know that a magnetical bar may have its magnetism very differently disposed; for it may have more than two poles, and the intermediate poles *cannot* have this disposition of the magnetism. Such a disposition is perhaps possible; but is by no means general, or even frequent. We

are disposed to think, that permanent magnetism must have its intensity diminishing in the very extremity of the bar. The reader may guess at our reasons from what is said in ELECTRICITY, § 222.

266. The following very curious and instructive phenomenon was the first thing which greatly excited the curiosity of the writer of this article, and long puzzled him to explain it. Indeed it was his endeavours to explain it which gradually opened up to him the theory of the mutual action of magnets contained in these paragraphs, and first gave him occasion to admire the sagacity of Dr. Gilbert, and to see the connecting principle of the vast variety of observations and experiments which that philosopher had made. It seems owing to the want of this connecting principle, that a book so rich in facts should be so little read, and that so many of Dr. Gilbert's observations have been published by others as new discoveries.

Amusing himself in the summer 1758 with magnetic experiments, two large and strong magnets A and B (Plate III. fig. 6.), were placed with their dissimilar poles fronting each other, and about three inches apart. A small needle, supported on a point, was placed between them at D, and it arranged itself in the same manner as the great magnets. Happening to set it off to a good distance on the table, as at F, he was surprised to see it immediately turn round on its pivot, and arrange itself nearly in the opposite direction. Bringing it back to D restored it to its former position. Carrying it gradually out along DF, perpendicular to NS, he observed it to become sensibly more feeble, vibrating more slowly; and when in a certain point E, it had no polarity whatever towards A and B, but retained any position that was given it. Carrying it farther out, it again acquired polarity to A and B, but in the opposite direction; for it now arranged itself in a position that was parallel to NS, but its north pole was next to N, and its south pole to S.

This singular appearance naturally excited his attention. The line on which the magnets A and B were placed had been marked on the table, as also the line DF perpendicular to the former. The point E was now marked as an important one. The experiments were interrupted by a friend coming in, to whom such things were no entertainment. Next day, wishing to repeat them to some friends, the magnets A and B were again laid on the line on which they had been placed the day before, and the needle was placed at E, expecting it to be neutral. But it was found to have a considerable verticity, turning its north pole toward the magnet B; and it required to be taken farther out, toward F, before it became neutral. While standing there, something chanced to joggle the magnets A and B, and they instantly rushed together. At the same instant, the little magnet or needle turned itself briskly, and arranged itself, as it had done the day before, at F, quivering very briskly, and thus shewing great verticity. This naturally surprised the beholders; and we now found that, by gradually withdrawing the magnets A and B from each other, the needle became weaker—then became neutral—and then turned round on its pivot, and took the contrary position. It was very amusing to observe how the simply separating the magnets A and B, or bringing them together, made the needle assume such a variety of positions and degrees of vivacity in each.

The needle was now put in various situations, in respect to the two great magnets: namely, off at a side, and not in the perpendicular DF. In these situations, it took an inconceivable variety of positions which could not be reduced to any rule; and in most of them, it required only a motion of one of the great magnets for an inch or two, to make the needle turn briskly round on its pivot, and assume a position nearly opposite to what it had before.

But all this was very puzzling, and it was not till after several months, that the writer of this article having conceived the notion of the magnetic curves, was in a condition to ex-

plain the phenomena. With this assistance, however, they are very clear, and very instructive.

Nothing hinders us from supposing the magnets A and B perfectly equal in every respect. Let NHM, NEL, be two magnetic curves belonging to A; that is, such that the needle arranges itself along the tangent of the curve. Then the magnet P has two curves SGK, SEI, perfectly equal, and similar to the other two. Let the curves NHM and SGK intersect in C and F. Let the curves NEL and SEI touch each other in E.

The needle being placed at C, would arrange itself in the tangent of the curve KGS, by the action of B alone, having its north pole turned toward the south pole S of B. But, by the action of A alone, it would be a tangent to the curve NHM, having its north pole turned away from N. Therefore, by the combined action of both magnets, it will take neither of these positions, but an intermediate one, nearly bisecting the angle formed by the two curves, having its north pole turned toward B.

But remove the needle to F. Then, by the action of the magnet A, it would be a tangent to the curve FM, having its north pole toward M. By the action of B, it would be a tangent to the curve KFG, having its north pole in the angle MFG, or turned toward A. By their joint action, it takes a position nearly bisecting the angle GFM, with its north pole toward A.

Let the needle be placed in E. Then, by the action of the magnet A, it would be a tangent to the curve NEL, with its north pole pointing to F. But, by the action of B, it will be a tangent to SEI, with its north pole pointing to D. These actions being supposed equal and opposite, it will have no verticity, or will be neutral, and retain any position that is given to it.

The curve SEI intersects the curve NHM in P and Q. The same reasoning shews, that when the needle is placed at P, it will arrange itself with its north pole in the angle

SPH: but, when taken to Q, it will stand with its north pole in the angle EQM.

From these facts and reasonings we must infer, that, for every distance of the magnets A and B, there will be a series of curves, to which the indefinitely short needle will always be a tangent. They will rise from the adjoining poles on both sides, crossing diagonally the lozenges formedby the PRIMARY or SIMPLE curves, as in Plate III. fig. 6. These may be called COMPOUND or SECONDARY magnetic curves. Moreover, these secondary curves will be of two kinds, according as they pass through the first or second intersections of the primary curves, and the needle will have opposite positions when placed on them. These two sets of curves will be separated by a curve GEH, in the circumference of which the needle will be neutral. This curve passes through the points where the primary curves touch each other. We may call this the *line of neutrality* or inactivity.

We now see distinctly the effect of bringing the magnets A and B nearer together, or separating them farther from each other. By bringing them nearer to each other, the point E, which is now a point of neutrality, may be found in the *second* intersection (such as F) of two magnetic curves, and the needle will take a subcontrary position. By drawing them farther from each other, E may be in the *first* intersection of two magnetic curves, and the needle will take a position similar to that of C.

If the magnets A and B are not placed so as to form a straight line with their four poles, but have their axes making an angle with each other, the contacts and intersections of their attending curves may be very different from those now represented; and the positions of the needle will differ accordingly. But it is plain, from what has been said, that if we knew the law of action, and consequently the form of the primary curves, we should always be able to say what will be the position of the needle. Indeed, the consideration of the simple curves, although it was the mean of sug-

gesting to the writer of this article the explanation of those more complicated phenomena, is by no means necessary for this purpose. Having the law of magnetic action, we must know each of the eight forces by which the needle is affected, both in respect of direction and intensity; and are therefore able to ascertain the single force arising from their composition.

When the similar poles of A and B are opposed to each other, it is easy to see, that the position of the needle must be extremely different from what we have been describing. When placed anywhere in the line DF, between two magnets, whose north poles front each other in N and S, its north pole will always point away from the middle point D. There will be no neutral point E. If the needle be placed at P or Q, its north pole will be within the angle EPH, or FQI. This position of the magnets gives another set of secondary curves, which also cross the primary curves, passing diagonally through the lozenges formed by their intersection. But it is the other diagonal of each lozenge which is a chord to those secondary curves. They will, therefore, have a form totally different from the former species.

267. The consideration of this compounded magnetism is important in the science, both for explaining complex phenomena, and for advancing our knowledge of the great desideratum, the law of magnetic action. It serves this purpose remarkably. By employing a very small needle, the points of neutrality ascertain very nearly where the magnetic curves have a common tangent, and shews the position of this tangent. By placing the two magnets so as to form various angles with each other, we can, by means of these neutral points, know the position of the tangent in every point of the curve, and thus can ascertain the form of the curve, and the law of action with considerable accuracy. The writer of this article took this method; and the result confirmed him in the opinion, that it was in the inverse duplicate ratio of the distances. The chief (perhaps the only)

ground of error seemed to be the difficulty of procuring large magnets, having the action of each pole very much concentrated. Large magnets must be employed. He attempted to make such, consisting of two spherical balls, joined by a slender rod But he could not give a strong magnetism to magnets of this form, and was forced to make use of common bars, the poles of which are considerably diffused. This diffusion of the pole renders it very difficult to select with propriety the points from which the distances are to be estimated, in the investigation of the relation between the forces and distances.

He tried another method for ascertaining this so much desired law, which had also the same result. Having made a needle consisting of two balls joined by a slender rod, and having touched it with great care, so that the whole strength of its poles seemed very little removed from the centres of the balls, he counted the number of horizontal vibrations which it made in a given time by the force of terrestrial magnetism. He then placed it on the middle of a very fine and large magnet, placed with its poles in the magnetic meridian, the north pole pointing south. In this situation he counted the vibrations made in a given time. He then raised it up above the centre of the large magnet, till the distance of its poles from those of the great magnet were changed in a certain proportion. In this situation its vibrations were again counted. It was tried in the same way in a third situation, considerably more remote from the great magnet. Then, having made the proper reduction of the forces corresponding to the obliquity of their action, the force of the poles of the great magnet was computed from the number of vibrations. To state here the circumstances of the experiment, the necessary reductions, and the whole computations, would occupy several pages, and to an intelligent reader would answer little purpose. Mr. Lambert's excellent dissertation in the 22d vol. of the *Mem. de l'Acad. de Berlin*, will shew the prolixity and intricacy of this investi-

gation. Suffice it to say, that these experiments were the most consistent with each other of any made by the writer of this article, with the view of ascertaining the law of magnetic action; and it is chiefly from their result that he thinks himself authorised to say, with some confidence, that it is inversely as the square of the distance. These experiments were first made in a rough way in 1769 and 1770. In 1775, observing that Mr. Æpinus seemed to think the action inversely as the distance (see his *Tentam. Theor. Electr. et Magn.* § 301. &c.), they were repeated with very great care; and to these were added another set of experiments, made with the same magnet and the same needle, placed not above the magnet, but at one side (but always in the line through the centre, perpendicular to the axis, so that the actions of the two poles might be equal). This disposition evidently simplifies the process exceedingly. The result of the whole was still more satisfactory. This conclusion is also confirmed by the experiments of Mr. Coulomb in the Memoirs of the Academy of Sciences at Paris for 1786 and 1787. It would seem therefore to be pretty well established. Another method, which seems susceptible of considerable accuracy, still remains to be tried. It will be mentioned in due time.

Such then are the general laws observed in the mutual action of magnets. We think it scarcely necessary to enter into a farther detail of their consequences, corresponding to the innumerable varieties of positions in which they may be placed with respect to each other. We are confident, that the sensible actions will always be found agreeable to the legitimate consequences of the general propositions which we have established in the preceding paragraphs. We proceed therefore to consider some physical facts not yet taken notice of, which have great influence on the phenomena, and greatly assist us in our endeavours to understand something of their remote cause.

268. Magnetism, in all its modifications of attraction, repulsion, and direction, is, in general, of a temporary or perishing nature. The best loadstones and magnets, unless kept with care, and with attention to certain circumstances, are observed to diminish in their power. Natural loadstones, and magnets made of steel, tempered as hard as possible, retain their virtue with the greatest obstinacy, and seldom lose it altogether, unless in situations which our knowledge of magnetism teaches us to be unfavourable to its durability. Magnets of tempered steel, such as is used for watch-springs, are much sooner weakened, part with a greater proportion of their force by simple keeping, and finally retain little or none. Soft steel and iron lose their magnetism almost as soon as its producing cause is removed, and cannot be made to retain any sensible portion of it, unless their metallic state suffer some change.

1. Nothing tends so much to impair the power of a magnet as the keeping it in an improper position. If its axis be placed in the magnetic direction, but in a contrary position, that is, with the north pole of it where the south pole tends to settle, it will grow weaker from day to day; and unless it be a natural loadstone, or be of hard tempered steel, it will, after no very long time, lose its power altogether.

2. This dissipation of a strong magnetic power is greatly promoted by heat. Even the heat of boiling water affects it sensibly; and if the magnet be made red hot, its power is entirely destroyed. This last fact has long been known. Dr. Gilbert tried it with many degrees of violent heat, and found the consequences as now stated; but having no thermometers in that dawn of science, he could not say any thing precise. He only observes, that it is destroyed by a heat not sufficient to make it visible in a dark room. Mr. Canton found even boiling water to weaken it; but on cooling again the greatest part was recovered*.

* M. Gay Lussac observed the same fact with regard to nickel; and he found also that both nickel and iron cease to be attracted by the magnet when they are made red hot. Ed.

3. What is more remarkable, magnetism is impaired by any rough usage. Dr. Gilbert found, that a magnet which he had impregnated very strongly, was very much impaired by a single fall on the floor; and it has been observed since his time, that falling on stones, or receiving any concussion which causes the magnet to ring or sound, hurts it much more than beating it with any thing soft and yielding. Grinding a natural loadstone with coarse powders, to bring it into shape, weakens it much; and loadstones should therefore be reduced into a shape as little different from their natural form as possible; and this should be done briskly, cutting them with the thin disks of the lapidary's wheel, cutting off only what is necessary for leaving their most active parts or poles as near their extremities as we can.

All these causes of the diminution of magnetism are more operative if the magnet be all the while in an improper position.

4. Lastly, magnetism is impaired and destroyed by placing the magnet near another magnet, with their similar poles fronting each other. We have had occasion to remark this already, when mentioning the experiments made with magnets in this position, for ascertaining the general laws or variations of their repulsion. We there observed, that magnets so situated always weakened each other, and that a powerful magnet often changed the species of the nearest pole of one less powerful. This change is recovered, in part at least, when it has taken place in a loadstone or a magnet of hard steel; but in spring tempered steel the change is generally permanent, and almost to the full extent of its condition while the magnets are together. It is to be remarked, that this change is gradual; and is expedited by any of the other causes, particularly by heat or by knocking.

269. On the other hand, magnetism is acquired by the same means, when some other circumstances are attended to.

1. A bar of iron, which has long stood in the magnetic direction, or nearly so, will gradually acquire magnetism, and the ends will acquire the polarity corresponding to their

situation. In this country, and the north of Europe, the old spindles of turret vanes, old bars of windows, &c. acquire a sensible magnetism; their lower extremity becoming a north pole, and the other end a south pole. Gilbert says, that this was first observed in Mantua, in the vane spindle of the Augustine church*—"*Vento flexa* (says he) *deprompta, et apothecario cuidam concessa, attrahebat ferrea ramenta, vi perquam insigni.*" The upper bar of a hand rail to a stair on the north side of the highest part of the steeple of St. Giles' church in Edinburgh is very magnetical; and the upper end of it, where it is lodged in the stone, is a vigorous south pole. It is worth notice, that the parts of such old bars acquire the strongest magnetism when their metallic state is changed by exposure to the air, becoming foliated and friable. It would be worth while to try, whether the æthiops martialis, produced by steam in the experiments for decomposing water, will acquire magnetism during its production. The pipe and the wires, which are converted into the shining æthiops, should be placed in the magnetic direction.

2. If a bar of steel be long hammered while lying in the magnetic direction, it acquires a sensible magnetism (See Dr. Gilbert's plate, representing a blacksmith hammering a bar of iron in the magnetic direction). The points of drills, especially the great ones, which are urged by very great pressure; and broaches, worked by a long lever, so as to cut the iron very fast, acquire a strong magnetism, and the lower end always becomes the north pole (*Phil. Trans.* xx. 417.) Even driving a hard steel punch into a piece of iron, gives it magnetism by a single blow. In short, any very violent squeeze given to a piece of tempered steel renders it magnetic, and its polarity corresponds with its position during the experiment. We can scarcely take up a cutting or boring tool in a smith's shop that is not magnetical. Even soft steel

* Gassendi is said to have first observed this in the cross of the spire of the church of St. Jean d'Aix, in Provence. Ed.

and iron acquire permanent magnetism in this way. Iron also acquires it by twisting and breaking. It is therefore difficult to procure pieces of iron or steel totally void of determinate and permanent magnetism; and this frequently mars the experiments mentioned in the first paragraphs of this article*. The way therefore to ensure success in these experiments is to deprive the rods of their accidental magnetism, by some of the methods mentioned a little ago. Let them be heated red hot, and allowed to cool while lying in a direction perpendicular to the magnetic direction (nearly E. N. E. and W S. W. in this country.)

3. As heat is observed to destroy magnetism, so it may also be employed to induce it on substances that are susceptible of magnetism. Dr. Gilbert makes this observation in many parts of his work. He says, that the ores of iron which are in that particular metallic state which he considers as most susceptible of magnetism, will acquire it by long continuance in a red heat, if laid in the magnetic direction, and that their polarity is conformable to their position, that end of the mass which is next the north becoming the north pole. He also made many experiments on iron and steel bars exposed to strong heats in the magnetical direction. Such experiments have been made since Gilbert's time in great number. Dr. Hooke, in 1684, made experiments on rods of iron and steel one fifth of an inch in diameter, and seven inches long. He found them to acquire permanent magnetism by exposure to strong heat in the magnetic direction, and if allowed to cool in that direction. But the magnetism thus acquired by steel rods was much stronger, and more permanent, if they were suddenly quenched with cold water, so as to temper them very hard.

* M. Gay Lussac was hence led to try if iron experienced any change of bulk in becoming magnetic. Having filled an iron tube with water, and attached to one end a very fine tube of glass in which the water rose, he magnetised the iron tube, but could not perceive any rise of the water in the glass tube. This is precisely what we should have expected, as we conceive that one of the poles is in a state of expansion, while the other is in a state of contraction. See Biot's *Traité de Physique*, tom. iii. p. 8. and *Phil. Trans.* 1816. p. 98, 105, 156, &c.

He found that the end which was next to the north, or the lower end of a vertical bar, was always its permanent north pole. Even quenching the upper end, while the rest was suffered to cool gradually, rendered it a very sensible south pole. No magnetism was acquired if this operation was performed on a rod lying at right angles to the magnetical direction.

In these trials the polarity was always estimated by the action on a mariner's needle, and the intensity of the magnetism was estimated by the deviation caused in this needle from its natural position. Dr. Gilbert made a very remarkable observation, which has since been repeated by Mr. Cavallo, and published in the Philosophical Transactions as a remarkable discovery. Dr. Gilbert says, p. 69. "*Bacillum ferreum, nihil ignitum appone versorio excito; stat versorium, nec ad tale ferrum convertitur: sed statim ut primum de candore aliquantulum remiserit, confluit illico.*" In several other parts of his treatise he repeats the same thing with different circumstances. It appears, therefore, that while iron is red hot, it is not susceptible of magnetism, and that it is during the cooling in the magnetic direction that it acquires it. Gilbert endeavoured to mark the degree of heat most favourable for this purpose: but being unprovided with thermometers, he could not determine any thing with precision. He says, that the versorium, or mariner's needle, was most deranged from its natural position a little while after the bar of iron ceased to shine in day-light, but was still pretty bright in a dark room. But there are other experiments which we have made, and which will be mentioned by and bye; by which it appears, that although a bright red or a white heat makes iron unsusceptible of magnetism while in that state, it predisposes it for becoming magnetical. When a bar of steel was made to acquire magnetism by tempering it in the magnetical direction, we found that the acquired magnetism was much stronger when the bar was made first of all very hot, even although allowed to come to its most magnetical state before quenching, than if

it had been heated only to that degree; nay, we always found it stronger when it was quenched when red hot*. We offer no explanation at present; our sole business just now being to state facts, and to generalize them, in the hopes of finding some fact which shall contain all the others.

4. The most distinct acquisitions and changes of magnetism are by juxtaposition to other magnets and to iron. As the magnetism of a loadstone or magnet is weakened by bringing its pole near the similar pole of another magnet, it is improved by bringing it near the other pole; and it is always improved by bringing it near any piece of iron or soft steel.

But this action, and the mutual relation of magnets and common iron, being the most general, and the most curious and instructive of all the phenomena of magnetism, they merit a very particular consideration,

Of the communication of Magnetism.

270. THE whole may be comprehended in one proposition, which may be said to contain a complete theory of magnetism.

Fundamental proposition.

Any piece of iron, when in the neighbourhood of a magnet, is a magnet, and its polarity is so disposed that the magnet and it mutually attract each other.

The phenomena which result from this fundamental principle are infinitely various, and we must content ourselves with describing a simple case or two, which will sufficiently enable the reader to explain every other.

271. Take a large and strong magnet NAS (Plate III. fig. 7.), of which N is the north, and S the south pole. Let it be properly supported in a horizontal position, with its

* A very interesting series of experiments on the influence of temperature upon magnets, was found, after the death of Coulomb, among his unpublished papers. An account of them will be found in Biot's *Traité de Physique*, tom. iii. p. 106, or in the article MAGNETISM in the EDINBURGH ENCYCLOPÆDIA, written by that eminent mathematician. ED.

poles free, and at a distance from iron or other bodies. Take any small piece of common iron, not exceeding two or three inches in length, such as a small key. Take also another piece of iron, such as another smaller key, or a bit of wire about the thickness of an ordinary quill.

1. Hold the key horizontally, near one of the poles, (as shewn at N° 1. fig. 7. Plate III.), taking care not to touch the pole with it; and then bring the other piece of iron to the other end of the key (it is indifferent which pole is thus approached with the key, and which end of the key is held near the pole.) The wire will hang by the key, and will continue to hang by it, when we gradually withdraw the key horizontally from the magnet, till, at a certain distance, the wire will drop from the key, because the magnetism imparted from this distance is too weak. That this is the sole reason of its dropping, will appear by taking a shorter, or rather a slenderer, bit of wire, and touch the remote end of the key with it: it will be supported, even though we remove the key still farther from the magnet.

2. Hold the key *below* one of the poles, as at N° 2, or 3, and touch its remote end with the wire. It will be suspended in like manner, till we remove the key too far from the magnet.

3. Hold the key *above* the poles, as at N° 4, or 5, and touch its adjacent end with the wire (taking care that the wire do not also touch the magnet). The wire will still be supported by the key, till both are removed too far from the magnet.

Thus it appears, that in all these situations the key has shewn the characteristic phenomenon of Magnetism, namely, attraction for iron. In the experiment with the key held above the pole, the wire is in the same situation in respect to magnetism as the key is when held below the pole; but the actions are mutual. As the key attracts the wire, so the wire attracts the key.

If the magnet be supported in a vertical position, as in Plate III. fig. 8. the phenomena will be the same; and when

the key is held directly above or directly below the pole, it will carry rather a heavier wire than in the horizontal position of the magnet and key.

Instead of approaching the magnet with the key and wire, we may bring the magnet toward them, and the phenomena will be still more palpable. Thus, if the bit of wire be lying on the table, and we touch one end of it with the key, they will shew no connection whatever. While we hold the key very near one end of the wire, bring down the pole of a magnet toward the key, and we shall then see the end of the wire rise up and stick to the key, which will now support it. In like manner, if we lay a quantity of iron fillings on the table, and touch them with the key, in the absence of the magnet, we find the key totally inactive. But, on bringing the magnet any how near the key, it immediately attracts the iron filings, and gathers up a heap of them.

272. In the next place, this vicinity of a magnet to a piece of iron gives it a directive power. Let NAS (Plate III. fig. 9.) be a magnet, and BC (N° 1.) a key held near the north pole, and in the direction of the axis. Bring a very small mariner's needle, supported on a sharp point, near the end C of the key which is farthest from N. We shall see this needle immediately turn its south pole towards C, and its north pole away from C. This position of the needle is indicated at *c*, by marking its north pole with a dart, and its south with a cross. Thus it appears that the key has got a directive power like a magnet, and that the end C is performing the office of a north pole, attracting the south pole of the needle, and repelling its north pole. It may indeed be said, that the needle at *c* arranges itself in this manner by the directive power of the magnet; for it would take the same position although the key were away. But if we place the needle at *b*, it will arrange itself as there represented, shewing that it is influenced by the key, and not (wholly at least) by the magnet. In like manner, if we place the needle at *a*, we shall see it turn its north pole to-

ward B, notwithstanding the action of the magnet on it. This action evidently tends to turn its north pole quite another way; but it is influenced by B, and B is performing the office of a south pole.

In like manner, if we place the key as at N° 2, we shall observe the end B attract the south pole of the needle placed at *a*, and the end C attract the north pole of a needle placed in *b*. In this situation of the key, we see that B performs the office of a north pole, and C performs the office of a south pole.

Thus it appears that the key in both situations has become a magnet, possessed of both an attractive and a directive power. It has acquired two poles.

273. Lastly, the magnetism of the key is so disposed, that the two magnets NAS and BC must mutually attract each other; for their dissimilar poles front each other. Now, it is a matter of uniform and uncontradicted observation, that when a piece of iron is thus placed near a magnet, and the disposition of its magnetism is thus examined by means of a mariner's needle, the disposition is such that two permanent magnets with their poles so disposed must attract each other. The piece of iron, therefore, having the same magnetic relation to the magnet that a similar and similarly disposed magnet has, must be affected in the same manner. We cannot, by any knowledge yet contained in this article, give any precise intimation in what way the polarity of the piece of iron will be disposed. This depends on its shape as much as on its position. By describing two or three examples, a notion is obviously enough suggested, which, although extremely gratuitous, and perhaps erroneous, is of service, because it has a general analogy with the observed appearances.

If one end of a slender rod or wire be held near the north pole of the magnet, while the rod is held in the direction of the axis (like the key in Plate III. fig. 7. N° 1.) the near end becomes a south, and the remote end a north pole. Keeping this south pole in its place, and turning the rod in

any direction from thence, as from a centre, the remote end is always a north pole. And, in general, the end of any oblong piece of iron which is nearest to the pole of a magnet becomes a pole of the opposite name, while the remote end becomes a pole of the same name with that of the magnet.

If the iron rod be held perpendicularly to the axis, with its middle very near the north pole of the magnet, the two extremities of the iron become north poles, and the middle is a south pole.

If the north pole of a magnet be held perpendicular to the centre of a round iron plate, and very near it, this plate will have a south pole in its centre, and every part of its circumference will have the virtue of a north pole.

If the plate be shaped with points like a star, each of these points will be a very distinct and vigorous north pole.

Something like this will be observed in a piece of iron of any irregular shape. The part immediately adjoining to the north pole of the magnet will have the virtue of a south pole, and all the remote protuberances will be north poles.

The notion naturally suggested by these appearances is, that the virtue of a north pole seems to reside in something that is moveable, and that it is protruded by the north pole of the magnet toward the remote parts of the iron; and is thus constipated in all the remote edges, points, and protuberances, much in the same manner as electricity is observed to be protruded to the remote parts and protuberances of a conducting body by the presence of an overcharged body. This notion will greatly assist the imagination; and its consequences very much resemble what we observe.

As a farther mark of the complete communication of every magnetic power by mere vicinity to a magnet, we may here observe, that the wire D of Plate III. fig. 7. N° 2, and 3, will support another wire, and this another; and so on, to a number depending on the strength of the magnet. The key has therefore become a true magnet in every respect; for it induces complete magnetism on the appended wire. That this is not the same operation of the great magnet (at

least not wholly so), appears by examining the magnetism of D with the needle, which will be seen to be more influenced by D than by A. This fact has been long known. The ancients speak of it: They observe, that a loadstone causes an iron ring to carry another ring, and that a third; and so on, till the string of rings appears like a chain.

274. What has now been said will explain a seeming exception to the universality of the proposition. If the key be held in the situation and position represented in Plate III. fig. 10. the bit of wire will not be attracted by it; and we may imagine that it has acquired no magnetism: But if we bring a mariner's needle, or a bit of wire, near to its remote end B, it will be strongly attracted, and shew B to be a north pole. The needle held near to C will also shew C to be a south pole. Also, if held near to D, it will shew D to be a north pole. Now the ends C, both of the key and of the wire, being south poles, they cannot attract each other, but, on the contrary, they will repel; and therefore the wire will not adhere to the key. And if the key of Plate III. fig. 7. N° 4. with the wire hanging to it, be gradually carried outward, beyond the north pole of the magnet, and then brought down till its lower end be level with the pole, the wire will drop off.

There is, however, one exception to the proposition. If the key in Plate. III. fig. 7. with its appending wire D, be gradually carried from any of the situations 2, 3, 4, or 5, toward the middle of the magnet, the wire will drop off whenever it arrives very near the middle. If we suppose a plane to pass through the magnetic centre A, pependicular to the axis (which plane is very properly called the magnetic equatorial plane by Gilbert), a slender piece of iron, held any where in this plane, acquires no sensible magnetism. It gives no indication of any polarity, *and it is not attracted by the magnet.* It is well known, that the activity of a loadstone or magnet resides chiefly in two parts of it, which have been called its poles; and that those are the best magnets or loadstones in which this activity is least diffused;

and that a certain circumference of every loadstone or magnet is wholly inactive. When a loadstone or magnet of any shape is laid among iron filings, it collects them on two parts only of its surface, and between these there is a space all round, to which no filings attach themselves.

We presume that the reader already explains this appearance to himself. Many things shew a contrariety of action of the two poles of a magnet. We have already observed that the north pole of a strong magnet will produce a strong northern polarity in the remote end of a small steel bar; and, if it be then applied near to that end in the opposite direction, it will destroy this polarity. In whatever these actions may consist, there is something not only different but opposite. They do not blend their effects, as the yellow and blue making rays do in producing green. They oppose each other, like mechanical pressures or impulsions. We have every mark of mechanical action ; we have local motion, though unseen, except in the gradual progression of the magnetical faculties along the bar ; but we have it distinctly in the ultimate effect, the approach or recess of the magnets : and in these phenomena we see plainly, that the forces in producing their effects, act in opposite directions. Whatever the internal invisible motions may be, they are composed of motions whose equivalents are the same with the equivalents of the ultimate, external, sensible motions ; therefore the internal motions are opposite and equal if the sensible motions are so, and conversely.

Adopting this principle, therefore, that the actions of the two poles are not only different but opposite, it follows, that if they are also equal and act similarly, each must *prevent* the action of the other ; and that there will be a mechanical equilibrium—it may even be called a magnetical equilibrium. Therefore if every part of a slender rod, or of a thin plate of iron, lie in the plane of the magnetic equator, the magnetic state (in whatever it may consist) cannot be produced in it. It will exhibit no magnetism ; have no polar faculties ; and we can see no reason why it should be attracted by the

magnet, or should attract iron. We must not forget to observe in this place, that iron in a state of incandescence acquires no magnetism by juxtaposition. We have already remarked, that iron in this state does not affect the magnet. If a bar of red hot iron be set near a mariner's needle, it does not affect it in the smallest degree till it almost ceases to appear red hot in day light, as has been observed by Dr. Gilbert. All actions that we know are accompanied by equal and opposite re-actions; and we should expect, what really happens in the present case, namely, that red hot iron should not be rendered magnetical and attractable.

There is a very remarkable circumstance which accompanies the whole of this communication of magnetism to a piece of iron. It does not impair the power of the magnet; but, on the contrary, improves it. This fact was observed, and particularly attended to, by Dr. Gilbert. He remarks, that a magnet, in the hands of a judicious philosopher, may be made to impart more magnetism than it possesses to each of ten thousand bars of steel, and that it will be more vigorous than when the operations began. A magnet (says he) may be spoiled by injudicious treatment with other magnets, but never can touch a piece of common iron without being improved by it. He gives a more direct proof. Let a magnet carry as heavy a lump of iron as possible by its lower pole. Bring a great lump of iron close to its upper pole, and it will now carry more. Let it be loaded with as much as it can carry while the lump of iron touches its upper pole. Remove this lump, and the load will instantly drop off. But the following experiment shews this truth in the most convincing manner:

Let NAS (Plate III. fig. 11.) be a magnet, not very large nor of extreme hardness. Let CD be a strong iron wire, hanging perpendicularly from a hook by a short thread or loop. The magnet, by its action on CD, renders D a north pole and C a south pole, and the polarity of D's magnetism fits it for being attracted. Let it assume the position C c, and let this be very carefully marked. Now bring a great

bar of iron *s* B *n* near to the other end of the magnet. We shall instantly perceive the wire C *e* approach to the south pole of the magnet, taking a position C *f*. Withdraw the bar of iron, and C *f* will fall back into the position C *e*. As we bring the iron bar gradually nearer to the magnet, the wire will deviate farther from the perpendicular, and when the bar B touches the magnet, CD will start a great way forward. It is also farther to be observed, that the larger the bar of iron is, the more will CD deviate from the perpendicular.

Now this must be ascribed to the action of the bar on the magnet. For if the magnet be removed, the bar alone will make no sensible change on the position of the wire. We know that the bar of iron becomes magnetical by the vicinity of the magnet. If we doubt this, we need only examine it by means of a piece of iron or a mariner's needle. This will shew us that *s* has become a south, and *n* a north pole. Here then are two magnets with their dissimilar poles fronting each other. In conformity with the whole train of magnetical phenomena, we must conclude that they attract each other, and must improve each other's magnetism.

276. This is a most important circumstance in the theory of magnetism. For it shews us, that, in rendering a piece of iron magnetic, there is no material communication. There is no indication of the transference of any substance residing in the magnet into the piece of iron; nor is there even any transference of a power or quality. Were this the case, or if the substance or quality which was in A be now transferred to B, it can no longer be in A; and therefore the phenomena resulting from its presence and agency must be diminished. We must say that the magnet has excited powers inherent, but dormant, in the iron; or is, at least, the occasion of this excitement, by disturbing, in some adequate manner, the primitive condition of the iron. We must also say that the competency of the magnet and of the iron to produce the phenomena, is owing to the same circumstances

in both; because we see nothing in the phenomena which authorises us to make any distinction between them. Whatever therefore causes one magnet to attract another, is also the reason why a piece of iron in the neighbourhood of a magnet attracts another piece of iron; and we must say that the cause of polarity, or the origin of the directive power, is the same in both. Now we understand perfectly the directive power of a magnet, as exerted on another magnet. We see that it arises from a combination and mechanical composition of attractions and repulsions. It must be the same in this magnetism now inherent in the iron. The piece of iron directs a mariner's needle, as a magnet would direct it; therefore, as there is something in a piece of iron, which *now* attracts something in another piece of iron, so ther is something in the first which repels something in the last.

277. It may indeed be said that it is not a piece of iron, but a mariner's needle, or magnet, that is thus directed by our iron magnetised by vicinity to a magnet. This objection is completely removed by the most curious of all the facts which occur in this manner of producing magnetism. Take a piece of common iron, fashion it, and fit it up precisely like a mariner's needle, and carefully avoid every treatment that can make it magnetical. Set it on its pivot, and bring it near the north pole of a magnet, placing the end, made like the south pole of the needle, next to the north pole of the magnet. In short, place it by hand exactly as a real mariner's needle would arrange itself. It will retain that position. Now carry it round the magnet, along the circumference of a magnetic curve, or in any regular and continuous route This piece of iron will, in every situation, assume the very same position or attitude which the real magnetical needle would assume if in the same place, and it will oscillate precisely in the same way.

Here then it is plain, that there is no distinction of power between the magnetism of the iron and of the real needle. To complete the proof: Instead of approaching the magnet

with this iron needle, bring it into the vicinity of a piece of iron, which is itself magnetical only by vicinity to a magnet, it will arrange itself just as the real needle would do, with the sole difference, that it does not indicate the *kind* of polarity existing in the extremities of the iron, because either end of it will be attracted by them. And this circumstance leads us to the consideration of the only distinction between the magnetism of a loadstone or magnet, and that of common iron.

278. The magnetism of common iron is momentary, and therefore indifferent; whereas that of a magnet is permanent and determinate. When iron becomes magnetic in the way now mentioned, it remains so only while the magnet remains in its place; and when that is removed, the iron exhibits no signs of magnetism. Therefore when the north pole of a magnet has produced a south pole in the nearest end of an iron wire, and a north pole at its remote end, if we turn the magnet, and present its south pole, the nearest end of the wire instantly becomes a north pole, and the other a south pole; and this change may be made as often, and as rapidly, as we please. This is the reason which made us direct the experimenter on the iron needle to begin his operation, by placing the end marked for a south pole next to the north pole of the magnet. It becomes a real south pole in an instant, and acts as such during its perigrination round the magnet. But in any one of its situations, if we turn it half round with the finger, the end which formerly turned away from a pole of the magnet, will now turn as vigorously toward it. Therefore, in carrying the iron needle round the magnet, we directed the progress to be made in a continuous line, to avoid all chance of mistaking the polarities.

279. For all the reasons now adduced, we think ourselves obliged to say, that the magnetism produced on common iron by mere juxtaposition to a magnet, is generated without any *communication* of substance or faculty. The power of producing magnetical phenomena is not *shared* betwen the

magnet and the iron. We shall call it INDUCED MAGNETISM; MAGNETISM BY INDUCTION.

We have said that induced magnetism of common iron is quite momentary. This must be understood with careful limitations. It is strictly true only in the case of the finest and purest soft iron, free of all knots and hard veins, and therefore in its most metallic state. Iron is rarely found in a state so verypure and metallic; and even this iron will acquire permanent and determinate magnetism by induction, if it has been twisted or hammered violently, although not in the magnetic direction; also the changes produced (we imagine) on the purest iron by the action of the atmosphere make it susceptible of fixed magnetism. But the magnetism thus inducible on good iron is scarcely sensible, and of no duration, unless it has lain in the neighbourhood of a magnet for a very long while.

What has now been said of common iron, is also true of it when in the state of soft steel.

280. But any degree of temper that is given to steel makes a very important change in this respect. In the first place, it acquires magnetism more slowly by induction than an equal and similar piece of common iron, and finally acquires less. These differences are easily examined by the deviations which it causes in the mariner's needle from the magnetic meridian, and by its attraction.

When the inducing magnet is removed, some magnetism remains in the steel bar, which retains the polarity which it had in the neighbourhood of the magnet.

Steel tempered to the degree fit for watch springs acquires a strong magnetism, which it exhibits immediately on the removal of the magnet. But it dissipates very fast; and, in a very few minutes, it is reduced to less than one half of its intensity while in contact with the magnet, and not two-thirds of what it was immediately on removal from it. It continues to dissipate for some days, though the bar be kept with care; but the dissipation diminishes fast, and it retains at least one

third of its greatest power for any length of time, unless carelessly kept or injudiciously treated.

Steel tempered for strong cutting tools, such as chisels, punches, and drills for metal, acquires magnetism still more slowly by induction, and acquires less of it while in contact with the magnet; but it retains it more firmly, and finally retains a greater proportion of what it had acquired.

Steel made as hard as possible, is much longer in acquiring all the magnetism which simple juxtaposition can give to it. It acquires less than the former; but it retains it with great firmness, and finally retains a much greater proportion.

Such ores of iron as are susceptible of magnetism, are nearly like hard steel in these respects; that is, in the time necessary for their *greatest* impregnation, and in the durability of the acquired magnetism. They differ exceedingly in respect to the degree of power which they can attain by mere juxtaposition, and the varieties seem to depend on heterogeneous mixture. We must observe, that few ores of iron are susceptible of magnetism in their natural state. The ordinary ores, consisting of the metal in the state of an oxyd, and combined with sulphur, are not magnetizable while remaining in that state. Most ores require roasting, and a sort of cementation, in contact with inflammable substances. This matter i not well understood; but it would seem that complete metallization is far from being the most favourable condition, and that a certain degree of oxydation, and perhaps some other composition, yet unknown, make the best loadstones. But all this is extremely obscure. The late Dr. Gowin Knight made a composition which acquired a very strong and permanent magnetism, but the secret died with him. Dr. Gilbert speaks of similar compositions, in which ferrugineous clays were ingredients; but we know nothing of the state of the metal in them, nor their mode of acquiring magnetism.

281. It is of peculiar importance to remark that the acquisition of magnetism is gradual and progressive, and that the gradation is the more perceptible in proportion as the steel is of a harder temper. When a magnet is brought to one end of a bar of common iron, its remote extremity, unless exceedingly long, acquires its utmost magnetism immediately. But when the north pole of a magnet is applied to one end of a bar of hard steel, the part in contact immediately becomes a south pole, and the far end is not yet affected. We observe a north pole formed at some distance from the contact, and beyond this a faint south pole. These gradually advance along the bar. The remote extremity becomes first a faint south pole, and it is not till after a very long while (if ever) that it becomes a simple, vigorous, north pole. More frequently it remains a diffused and feeble north pole: nay, if the bar be very long, it often happens that we have a succession of north and south poles, which never make their way to the far end of the bar. This phenomenon was first observed (we think) by Dr. Brook Taylor, who gives an account of his observations in the *Philosophical Transactions*, No. 344.

282. From the account we have given of these phenomena of induced magnetism, it appears that the temporary magnetism is always so disposed that the sum of the mutual attractions of the dissimilar poles exceeds the sum of the repulsions between the similar poles, and that therefore the two magnets tend to each other. This is evidently equivalent to saying, that a piece of unmagnetic iron is always attracted by a magnetic. No exception has ever been observed to this fact: for Pliny's story of a Theamedes, or loadstone, which repels iron, is allowed by all to have been a fable.

We think ourselves authorised to say that this attraction of the loadstone for iron, or this tendency of iron to the loadstone, is a secondary phenomenon, and is the *consequence* of the proper disposition of the induced magnetism. The proofs

already given of the compound nature of this phenomenon, namely, that it arises from the excess of two attractions above two repulsions, need (we imagine) no addition. But the following considerations place the matter beyond doubt.

1. The magnetism of the two poles is evidently of an opposite nature; the one repelling what the other attracts. If the one attracts iron, therefore, the other should repel it. But each pole, by inducing a magnetism opposite to its own, on the nearest end of the iron, and the same with its own on the remote end, and its action diminishing with an increase of distance, there must always be an excess of attraction, and the iron must be attracted.

2. Each of the magnets A and B, in either of the positions represented in Plate III. fig. 12. would alone attract the piece of common iron C. But when placed together, the south pole of A tends to render the upper end of C a north pole; while the north pole of B tends to make it a south pole. If their actions be nearly equal, the weight of C cannot be supported by the magnetism induced by any difference of action that may remain. While C is hanging by B alone, let A be gradually brought near; it gradually destroys the action of the north pole of B, so that C gradually loses its magnetism and polarity, and its weight prevails.

3. In all those cases where the induction of magnetism is slow, the attraction is weak in proportion. This is particularly remarked by Dr. Gilbert. If we take pieces of common iron, and of steel of different tempers, but all of the same size and form, we shall find that the iron is much more strongly attracted than any of the rest, and that the attraction for each of them is weaker in proportion as they are harder. This diversity is so accurately observed, that when the piece is thoroughly susceptible of magnetism, we can tell, with considerable precision, what degree will be ultimately acquired, and how much will be finally retained. Also, the attraction of the magnet for any of those pieces of steel increases exactly in proportion as their acquired magnetism increases.

4. An ore of iron incapable of acquiring magnetism is not attracted by a magnet. But we know that, by cementation with charcoal dust, they may be rendered susceptible of magnetism. In this state they are attracted. It is an universal fact, that any substance that is attracted by a magnet may be rendered magnetical, and that none else can. We have already observed that red hot iron is not attracted: nor does it acquire any directive power while in that state. From all this we must conclude, that the previous induction of magnetism is the mean of the observed attraction of magnets for iron, and that this is not a primary fact in magnetism.

These observations also complete the proof that magnetic attraction and repulsion are equal at the same distance, and follow the same law. Dr. Gilbert seems to think, that the repulsion is always weaker than the attraction; and this is almost the only mistake in conception into which that excellent philosopher has fallen. But it only requires a fair comparison of facts to convince a good logician, that since, *in every case*, and at every distance, either pole of a magnet attracts either end of a piece of common iron, it is impossible that one of these forces can exceed the other. It might be so, were it not that induced magnetism is durable in proper substances. And if we take magnets which have been made such by induction, and present them to each other with their similar poles fronting each other, they never fail to repel each other at considerable distances, and even at very small distances for a few moments; and this is the case which ever poles are next each other. This cannot be on any other supposition. Cases would occur of polarity without attraction, or of attraction without polarity. Such have never been seen, any more than the Theamedes, always repelling iron.

283. Let a great number of small oblong pieces of iron be lying very near each other on the surface of quicksilver. Bring a strong magnet into the midst of them. It immediately renders them all magnetical by induction. The one

nearest the north pole of the magnet immediately turns one end toward it, and the other end away from it. The same effect is produced on the one that is just beyond this nearest one. Thus the remote end of the first becomes a north pole, and the nearest end of the second becomes a south pole. These, being very near each other, must mutually attract. The same thing may be said of a third, a fourth; and so on. And thus it appears, that not only is magnetism induced on them all, but also, that the magnetism of each is so disposed, that both ends of it are in a state of attraction for the ends of some of its neighbours; and that they will therefore arrange themselves by coalescence in some particular manner. Should a parcel of them chance to be standing with their centres in a magnetic curve, with their heads and points turned in any ways whatever, the moment that the magnet is brought among them, and set in the axis of that magnetic curve, the whole pieces of this row will instantly turn towards each other, and their ends will adhere together, if they are near enough; otherwise they will only point toward each other, forming a set of tangents to the magnetic curve, reaching from one pole of the magnet to the other.

Or, suppose a vast number of small bits of iron, each shaped like a grain of barley, a little oblong. Let them be scattered over the surface of a table, so near each other as just to have room to turn round. Let a magnet be placed in the midst of them. They will all have magnetism induced on them in an instant; and such as are not already touching others, will turn round (because they rest on the table by one point only), and each will turn its ends to the ends of its neighbours; and thus they will arrange themselves in curves, which will not differ greatly from true magnetic curves (because each grain is very short), issuing from one pole of the magnet, and terminating in the other.

Does not this suggest to the reflecting reader an explanation of that curious arrangement of iron filings round a magnet, which has so long entertained and puzzled both the phi-

losophers and the unlearned, and which has given rise to the Cartesian and other theories of magnetism? The particles of iron filings are little rags of soft iron torn off by the file, and generally a little oblong. These *must* have magnetism induced on them by a magnet, and, while falling through the air from the hand that strews them about the magnet, they are at perfect liberty to arrange themselves magnetically; and *must therefore so arrange themselves*, forming on the table curves, which differ very little indeed from the true magnetic curves. Suppose them scattered about the table before the magnet is laid on it. If we pat the table a little, so as to throw it into tremors, this will allow the particles to dance, and turn round on their points of support, till they coalesce by their ends in the manner already described.

All this is the genuine and inevitable consequence of what Dr. Gilbert has taught us of induced magnetism. It must be so; and cannot be otherwise. This curious arrangement of iron filings round a magnet is therefore not a primary fact, and a foundation for a theory, but the result of principles much more general.

284. Most of our readers know that this disposition of iron filings has given rise to the chief mechanical theories which have been proposed by ingenious men for the explanation of all the phenomena of magnetism. An invisible fluid has been supposed to circulate through the pores of a magnet, running along its axis, issuing from one pole, streaming round the magnet, and entering again by the other pole. This is thought to be indicated by those lines formed by the filings. The stream, running also through *them*, or around them, arranges them in the direction of its motion, just as we observe a stream of water arrange the flote grass and weeds. It would require a volume to detail the different manners in which those mechanicians attempt to account for the attraction, repulsion, and polarity of magnetic bodies, by the mechanical impulsion of this fluid. Let it suffice to

say, that almost every step of their theories is in contradiction to the acknowledged laws of impulsion. Nay, the whole attempt is against the first rule of all philosophical discussion, never to admit for an explanation of phenomena the agency of any cause which we do not know to exist, and to operate in the very phenomenon We know of no such fluid; and we can demonstrate, that the genuine effects of its impulsion would be totally unlike the phenomena of magnetism. But the proper refutation of these theories would fill volumes. Let it suffice (and to every logician it will abundantly suffice) to remark, that this phenomenon is but a secondary fact, depending on, and resulting from, principles much more general, viz. the induction of magnetism, and the attraction of dissimilar, and repulsion of similar, poles.

The above explanation of the curious disposition of iron filings round a magnet, occurred to the writer of this article while studying natural philosophy, on seeing the Professor exhibit Mr. Henshaw's beautiful experiment in proof of terrestrial magnetism. He at that time imagined himself the author, and promised himself some credit for the thought. But having seen the *Physiologia Nova de Magnete* by Dr. Gilbert, he found that it had not escaped the notice of that sagacious philosopher; as will appear past dispute from the following passage, as well as some others, less pointed in that work: "Magnetica frusta (that is, substances susceptible of magnetism) bene et convenienter intra vires posita, mutuo cohærent. Ferramenta, presente magnete (etiamsi magnetem non attingant), concurrunt, solicité se mutuo quærunt, et amplexantur, et, conjuncta, quasi ferruminantur. Scobs ferrea, vel in pulverem redacta, fistulis imposita chartaceis—supra lapidem meridionaliter locata, vel propius tantum admota, in unum coalescet corpus; et subito tam multæ partes concrescunt et combinantur; ferrumque aliud affectat conjuratorum turma et attrahit; ac si unum tantum et integrum esset ferri bacillum; dirigiturque supra

lapidem in septemtriones et meridiem. Set cum longius a magnete removeantur (tanquam soluta rursus) separantur, et diffluunt singula corpuscula." B. ii. c. 23.

Mr. Æpinus also had taken the same view of the subject. It is also very clearly conceived and expressed by the celebrated David Gregory, Savilian Professor of astronomy in the University of Oxford, in a MS. volume of notes and commentaries, written by him in 1693, on Newton's *Principia*, and used by Newton in improving the second edition. The MS is now in the library of the university of Edinburgh. Gregory's words are as follows: "Mihi semper dubium visum est num magnetica virtus mechanicé, *i. e.* per impulsum, producatur. Mirum est, effluvia, quæ ferrum agitare valent, bracteas aureas *interpositas* ne vel minimum a loco movere. Lucretii et Cartesii theoriam, de fugato intermedio aëre, refutat experimentum infra aquam institutum. *Sulci in limatura ferri, magneti in plano cujusvis meridiani circamposita, non fiunt ab effluviis secundum istos canales motis, sed ex inde, quod ipsa ramenta, magnetice excitata, sese secundum longitudinem et secundum polos disponunt.* Ex altera vero parte exinde quod vis magnetica, interveniente flamma aut calore, interrumpatur; quod virga ferrea, vel diuturno situ perpendiculari, vel in eo situ frigescendo, virtutem magneticam a tellure acquirat; utnos docet perspicacissimus Gilbertus: quod mallei super incudem ictu forti ad alterum extremum, virtutem acquirat magneticam; quod ictu forti vel saltem fortiori ad alterum extremum poli permutentur, ut qui prius septemtriones respiciebat nunc austrum respiciat; quod ictu forti ad medium, virtutem illam prorsus amittat: hæc inquam et similia, mechanicum ejus qualitatis ortum arguunt. Hugenius, præter gravitatem, etiam magneticam, et electricam virtutem, aliasque plures experimento novit vires naturales, ut mihi ipsi narravit hac æstate anni 1693. Qualis ut hæc forsitan quod cymba papyracea, prope labra vasis aquam, cui innatet, continentis, posita, la-

brum vicinissimum continuo, et cum impetu petat*. *Nat. MS. in Prop.* 23. ii. *Prin. Nor.*

285. Not only the mere arrangement of the filings in curve lines follows of neccessity from the properties of induced magnetism, but all the subordinate circumstances of this phenomenon are included in the same explanation. By continuing to tap the table, and throw it into tremors, the filings are observed to approach gradually, but very slowly, to the poles of the magnet. Each particle is a very small temporary magnet. The attractive power of the great magnet, $\overline{m - p} - \overline{n - q}$, is therefore extremely small in proportion to its directive power, $\overline{m + p} - \overline{n + q}$. And we observe that the accumulation of the filings round the poles of the magnet is so much the slower as the filings are finer.

286. If a paper be laid above the magnet, and the filings be sprinkled on it, we observe them to constipate along its edges, while none remain immediately above its substance; they are all beyond, or on the outside of its outline, and they are observed not to be lying flat on the paper, but to be standing obliquely on one point. They move off from the paper immediately above the magnet, because they repel each other. They stand obliquely from the edges, because that is the direction of a magnetic meridian at its parting from the pole. If the magnet be at some distance below the paper, then tapping the paper will cause the filings to move away from the magnet laterally. This singular and unexpected appearance is owing to the combination of gravity with the magnetic action. A particle, such as $n\,s$ (Plate III. fig. 12.), rests on the paper by the point n, which is a temporary north pole (S being supposed the south pole of the magnet). The particle takes a position $n\,s$ nearer to the horizon than the position $n\,o$, which it would take if its

* Perhaps it may be proper to observe, that Dr. Gregory expresses his differing in his opinion from Newton about magnetism. Newton, in this proposition thinks, that the law of magnetic action approaches to the inverse triplicate ratio of the distances. Dr. Gregory invalidates the argument used by Newton.

centre of gravity *b* were supported. The position is such, that its weight acting vertically at *b*, is in equilibrio with the magnetic repulsion *s d*, exerted between S and *s*. When the paper is tapped, it is beaten down, or withdrawn from *n*, and the particle of iron is left for a moment in the air. It therefore turns quickly round *b*, in order to assume a position parallel to *n o*, and it meets the paper, as that rises again after the stroke, in a point farther removed from the magnet, and again descends by its weight (turning round the newly supported point *n*), till it again takes a position parallel to *n s*, but farther off, as represented by the dotted line. Thus it travels gradually outwards from the magnet, appearing to be repelled, although it is really attracted by it. If the magnet be held above the paper, at a little distance, the filings, when we repeatedly pat the paper, gradually collect into a heap under it. This will appear very plainly to one who considers the situation of a particle in the manner now explained.

287. The curve lines formed by very fine filings approach very nearly to the form of the primary curve which indicates the law of magnetic action in the way already explained. If the magnet be placed under water, and if filings be sprinkled copiously on the surface of it from a gauze search, held at some distance above it, the resistance to their motion through the water gives them time to arrange themselves magnetically before they reach the bottom, and the lines become more accurate. But they were so much deranged by any method that we could take for removing the water, and measuring them, that we were disappointed in our expectations of obtaining a very near approximation to the law of action.

288. We took notice of some very singular phenomena of a compass needle in the neighbourhood of two magnets, and we observed that, in this case also, the needle was always a tangent to a curve of another kind, and which we called *secondary* and *compound magnetic curves*. These are produced in the same way, by strewing iron filings round

the magnets. Many representations have been given of these curves by different authors, particulary by Muschenbroek, in his *Essais de Physique;* and by Fuss in the *Comment. Petropolit.* Great use has been made of these arrangements of filings by two magnets in the theories of magnetism proposed by those who insist on explaining all motion by impulse. When the dissimilar poles of two magnets A and B (Plate III. fig. 13.) face each other, the curves formed by the filings considerably resemble those which surround a single magnet, and give the whole somewhat of the appearance of a magnet with very diffused poles. The arranging fluid, which streams from one pole of a magnet, is supposed to meet with no obstruction to its entry into the adjoining pole of the other magnet, but, on the contrary, to be impelled into it; and therefore (say the proposers) it circulates round both as one magnet, and by its vortex brings the magnets together; which phenomenon we call the attraction of the magnets. But when the similar poles front each other; for example, the poles from which the arranging fluid issues, then the two streams meet, obstruct each other, accumulate, and, by this accumulation, cause the magnets to recede from each other; which we call the repulsion of the magnets. This is the only explanation of this kind that can make any pretensions to probability, or indeed that can be conceived. For how the free circulation in the former case can bring the two magnets together, no person can form to himself any conception. We see nothing like this produced by any vortex that we are acquainted with. All such vortices cause bodies to separate. But even this explanation of magnetic repulsion is inadmissible. It will not apply to the repulsion of the receiving poles; and the phenomena of the filings are inconsistent with the notion of accumulation. The filings indeed accumulate, and they look not unlike two streams which oppose each other, and deflect to the sides (see Plate III. fig. 14.):

But, unfortunately, by tapping the paper gently, the filings do not move off from the magnets, but approach them much faster than in any other experiment. The phenomenon receives a complete and palpable explanation from the principles we have established. Both magnets concur in giving the same polarity to every particle of the filings. Thus, if the fronting poles are north poles, each particle has its nearest end made a vigorous south pole, and its remote end a north pole; and it is *therefore* strongly attracted towards both magnets, while it is arranged in the tangent to the secondary curve of that class, which crosses the others nearly at right angles.

289. Since it is found, that the magnetism, even of natural loadstones and hard steel, and still more those of softer tempered steel, are continually tending to decay; and since we find that it may be induced by mere approach to a magnet; and since we know that magnets may oppose each other in producing it—it is reasonable to suppose, that when a piece of iron has acquired a slight, though permanent magnetism, by the vicinity of a magnet, a magnet applied in the opposite direction will destroy it, and afterwards produce the opposite magnetism.

Accordingly, we may change the poles of soft magnets at pleasure.

Farther; since we find that loadstones and hard tempered steel bars are distinguished from soft ones only by the *degree* of obstinacy with which they retain their present condition, we should also expect that hard magnets will even affect each other. It must therefore happen, that a powerful magnet applied to a weak one, so that their similar poles are in contact, shall weaken, destroy, and even change the magnetism of the weaker. Dr. Knight's famous magazine of magnets enabled him to change the poles of the greatest and the strongest natural loadstone, or artificial magnet, that could be given him, in the space of one minute.

290. We now see clearly the reason why magnetic repulsion is weaker than attraction at the same distance. When magnets are placed with their similar poles fronting each ether, in order to make trials of their repulsion, they really do weaken each other, and are not in the same magnetical condition as before. For similar reasons, we see how experiments with magnets attracting each other rather improve them, and make their attractive powers appear greater than they are. All these effects must be most remarkable in soft magnets, especially when long.

291. We also see, that the observed law of attraction and repulsion between two magnets must be different from the real law of magnetic action. For, in the experiments made on attraction at different distances, beginning with the greatest distance, the magnetism is continually increasing, and the attraction will appear to increase in a higher rate than the just one: the contrary may happen, if we begin with the smaller distances. The results of experiments on repulsion must be still more erroneous; because it is easier to diminish any accumulation which required an exertion to produce it, than to push it still farther.

292. We have now a complete explanation of the remarkable fact, that the induction of magnetism does not weaken the magnet employed; but, on the contrary, improves it. The magnetism induced on the iron causes it to act on the magnet employed in the very same manner that a permanent magnet of the same shape, size, and strength, would do. Nay, it will have even a greater effect; for as it improves the magnet, its own induced magnetism will improve; and will therefore still farther improve the magnet.

293. Hence it is, that, in whatever manner a magnet touches a piece of iron, it improves by it. It may be hurt by a magnet in an improper position; but it always puts common iron into a state which increases its own magnetism. This has been known as long as magnetism itself;

and the ancients conceived the notion, that the magnet somehow fed upon the iron*.

We think that these observations authorise us to say, that in reducing a loadstone into a convenient shape, as much as possible of the operation should be performed by grinding them with emery, in cavities made in large blocks of *hammered* iron. The magnetism induced on the iron must be favourable to the conservation of that in the loadstone, which, we are persuaded, is rapidly dissipated by the tremors into which this very elastic substance is thrown by the grinding with coarse powders in any mould but iron. We imagine, that the cutting off slices by the lapidaries wheel has the same bad effect.

294. Not only will a magnet lift a greater lump of iron by its north pole, when another lump is applied to its south pole, but it will lift a greater piece of iron from an anvil than from a wooden table: for the magnet induces the properly disposed polarity, not only in the iron which it lifts, but also in the anvil, or any piece of iron immediately beyond it. This is so disposed as to increase the magnetism of the piece of iron between them; and therefore to increase their attraction. The magnetism induced on the anvil is also in part, and perhaps chiefly, induced by the intervening iron. These experiments are extremely variable in their results.—Sometimes a small magnet will pull an iron wire from a large and strong one. Sometimes this will be done even

* So Claudian.——"Nam ferro nutrit vitam, ferrique vigore
Vescitur: hoc dulces epulas, hoc pabula novit:
Hinc proprias renovat vires: hinc fusa per artus
Aspera secretum servant alimenta vigorem:
Hoc absente perit, tristi morientia torpent
Membra fame, venasque sitis consumit apertas."

Pliny says, "Sola hæc materia (ferrum) vires ab eo lapide accipit retinetque longo tempore, aliud apprehendens ferrum, ut annulorum catena spectetur interdum, quod imperitum vulgus ferrum appellat vivum.

by a piece of unmagnetic iron ; and the results appear quite capricious. But they are accurately fixed, depending on the induced compound magnetism. Mr. Æpinus has stated some of the more simple cases, in which we can tell which magnet shall prevail. But the unfolding even of these cases would take a great deal of room, and must be omitted here. Besides, we are too imperfectly acquainted with the degree of magnetism induced on the various parts of an iron rod, and the degree of magnetism inherent in the various parts of the magnets, to be able to say, with certainty, even in those simple cases, on which side the superiority of attraction will remain.

295. We may now proceed to deduce from this theory (for so it may justly be called, since all is reduced to one fact) the process for communicating magnetism to bodies fitted for receiving and retaining it; that is, the method of making artificial magnets. We acknowledge, that we do not know the internal process by which magnetism is induced, nor even in what this magnetism consists. All that we know is, that the bringing the pole of a magnet near to any magnetisable matter, produces a magnetism of the kind opposite to that of the pole employed. We know that this is the case with both poles, and that it obtains at all the distances where magnetism is observed. We know that the action of one pole is contrary to that of the other; that is, it counteracts the other, prevents it from producing its effect, and destroys it when already produced: and we know, that the production of these effects resembles in its result the protrusion of something fluid through the pores of the body, constipating it in all remote parts; as if the virtue of a pole resided in this moveable matter. This is nearly all that we know of it; and by these facts and notions we must judge of the propriety and effect of all the processes for magnetising bodies.

The most simple method of magnetising a steel bar, is to apply the north pole of a magnet to that end which we wish

to render a south pole. Attention to the effects of this application is very instructive. Have in readiness a very small compass needle, turning on its pivot. It should not exceed half an inch in length, and should be as hard tempered as possible, and strongly impregnated. Immediately after the application of the magnet, carry the needle along the side of the bar. If the bar be long, and very hard, we shall observe a south polarity at the place of contact; a north polarity at a small distance from it; beyond this a weak south polarity; then a weak and diffused north polarity, &c.; toward the remote end the polarity will be found very uncertain. The same thing may be discovered by laying a stiff paper on the bar, and sprinkling iron filings over it, and then gently tapping the paper, to make them arrange themselves in curve lines; which will point out the various poles, and shew whether they are diffused or constipated. It is very amusing and instructive to observe the progress of this impregnation. In a few minutes after the first application of the magnet, we shall perceive the state of magnetism very sensibly changed. The north pole will be farther from the magnet, and will be more distinct; the southern polarity will also be protruded, and may appear for a moment at the remote extremity. The change advances; but the progress is more slow, and at last is insensible. When the bar is not harder than the temper of a cutting tool, the process is soon over; and if the bar is but six or eight inches long, the remote end shews the north polarity in a very few minutes. When the bar is very hard, the progress of impregnation is greatly expedited by striking it so as to make it sound. If it be suspended by a string in a vertical position, and the magnet applied to its lower end, the striking it with a key will make it ring; and in this way make the progress of magnetization very quick: but it does not allow it to acquire all the magnetism that can be given it by a very strong magnet.

But this is a bad way of impregnation. It is seldom that uniform magnetism, with only two poles, and those of equal strength can be given Even when there are but two, the remote pole is generally diffused, and therefore feeble. It is much improved by employing two magnets, one at each end. And if the bar is not more than six or eight inches long, and good magnets are employed, the magnetism is abundantly regular. This, accordingly, is practised for the impregnation of dipping needles, which must not be touched, lest we disturb the centre of gravity of the needle. But in all cases, this method is tedious, and does not give strong magnetism.

The method which was usually practised before we had obtained a pretty clear knowledge of magnetism, was to apply the pole of a magnet to one end of the bar, and pass it along to the other end, pressing moderately. This was repeated several times on both sides of the bar, always beginning the stroke at the same end as at first, and, in bringing the magnet back to that end, keeping it at a distance from the bar. The effect of this operation was to leave the end at which we began the stroke possessed of the polarity of the pole employed.

A general notion of the process may be given as follows, observing, however, that there occur very many great and capricious anomalies. When the north pole N (Plate IV. fig. 2.) of the magnet A is set on the end C of the bar CBD, a south pole is produced at C, and a north pole at D, when the length of the bar is moderate. As the magnet advances slowly along the bar, the southern polarity at C first increases, then diminishes, and vanishes entirely when N has arrived at a certain point *a*; after which, a northern polarity appears at C, and increases during the whole progress of the magnet. In the mean time, the northern polarity first produced at D increases till the magnet reaches a certain point *e*, then diminishes, vanishes when the mag-

net reaches a certain point *f*; after which, a southern polarity appears at D, which increases till the magnet reaches D. Mr. Brugmann, who first attended minutely to these particulars (for Gilbert speaks of them pointedly), calls *a* and *f points of indifference*, and *e* the *culminating* point of the pole D, and *i* the culminating point of the pole C. Hardly can any general rule be given for the situation of these points, nor even for the order in which they stand; so great and capricious are the anomalies in an amazing series of experiments narrated by Brugmann and by Van Swinden. Repeating the operation, and beginning at C, the northern polarity there is weakened (sometimes destroyed), then restored, and continually increased during the rest of the stroke. The southern polarity at D is also first weakened, and sometimes destroyed; then restored, and finally augmented. The points *i*, *a*, *e*, *f*, change their situations, and frequently their order.

Van Swinden has attempted to deduce some general laws from his immense list of experiments, avoiding every consideration of a hypothesis, or the least conjecture by what means these faculties are excited. But though we have perused his investigation with care and candour, we must acknowledge, that we have not derived any knowledge which can help us to predict the result of particular modes of treatment with any greater precision than is suggested by a sort of common sense, aided (or perhaps perverted) by a vague notion, that these energies reside in something, which avoids the pole of the same name, carrying along with it this distinctive energy or polarity. This conception tallies perfectly with these observations of Brugmann and Van Swinden; and admits of all the anomalies in the situation of Brugmann's indifferent and culminating points, if we only suppose that this motion is obstructed by the particles of the body. We must leave this to the reflection of the reader, who will *guess* how, when the magnet is between C and *i*,

this substance, avoiding the pole N of the magnet, escapes below it, and goes toward the farther end. As the magnet advances, it drives some of this back again, &c. &c. This is gratuitous; but it aids the fancy, which, without some conception of this kind, has no object of steady contemplation. We have no thought, when we speak of the generating at C, or *a*, or *e*, a faculty of some kind, by the exertion of the same faculty in N. The conception is too abstracted, and much too complex. We must content ourselves with knowing, that N produces a south pole immediately under it, and a north pole everywhere else, or endeavours to do so. It is unnecessary to insist longer on this method: Common sense shews it to be a very injudicious one.

This method was greatly improved by beginning the friction at the centre. Apply the north pole at the centre or middle of the bar, and draw it over the end intended for the south pole. Having done this several times to one end on both sides, turn the magnet, applying its south pole to the middle of the bar, and drawing it several times over the end intended for the north pole.

It was still more improved by employing two magnets at once, placed as in Plate IV. fig. 3. on the middle B of the bar, and drawing them away from each other, over the ends of it, as shewn by the directing darts, and repeating this operation. It is plain that, as far as we understand any thing of this matter, this process must be much preferable to either of the former two. The magnets A and E certainly concur in producing a properly disposed magnetism on all that lies between them; and therefore on the whole bar at the end of each stroke. The end C must become a north, and D a south pole. Still, however, as the stroke goes on to the point of indifference, each magnet tends to weaken the polarity of the parts situated beyond it.

This method continued to be practised till about the year 1750. Mr. Canton, availing himself of the experiments of

Mr. Mitchell of Cambridge, published his method by the DOUBLE TOUCH, as it is called. See *Monthly Review* for 1785.

296. We need not repeat the defects of this method, which are found in every treatise on magnetism; we shall only make some observations on the peculiar advantages of the process, as prescribed by Mitchell, Canton, and improved by Mr. Antheaume, in his memoir *sur les Aimans artificiels* 1766, which was crowned by the Academy of Sciences. (See also dissertations on the subject by *Le Maire and Du Hamel*, 1745.)

There is an evident propriety in the arrangement invented by Mr. Mitchell, represented in Plate IV. fig. 4. The magnetism induced on the two pieces of soft iron AD and BC is an excellent method for *securing* every accession of magnetism to either of the bars. A good deal depends on the proper size and length of these pieces; and our ignorance of the interior process obliges us to have recourse to experiment alone for ascertaining this. Whatever circumstances induce the strongest magnetism on those pieces of iron, will cause them to produce the greatest effect on the steel bars; and this will be indicated by a greater attraction. Therefore that distance will be the best which enables two bars AB and DC to lift the greatest weight hung on the piece AD or BC. When we impregnated bars whose breadth was about one-tenth of their length, and their thickness about one-half of their breadth, we found, that if AD was about one fourth, or nearly one third, of AB, they carried more than if it was either much longer or much shorter. Mr. Antheaume's addition of the two great bars of iron E and F makes a sensible improvement of the *beginning* of the impregnation, when very weak magnets are employed; but did not seem to us to be of any farther service on the table. This is agreeable to any theory which can be established by what we have said hitherto.

The method of employing the magnets A and E (Plate IV. fig. 5.), prescribed by Mitchell and Canton, is extremely judicious. The meeting of the dissimilar poles at top increases the magnetism of each. The two dissimilar poles F and G, certainly tend to give a regular and proper magnetism to the part FG of the bar which lies between them; and this is the case on whatever part of the bar they are placed. But each pole tends to destroy the present magnetism of what lies between it and the pole of the bar on that side. But mark—they tend to produce the desired magnetism on what lies between them with the *sum* of their forces; while each tends to destroy the magnetism of the part without it by the *difference* only of their forces. Therefore, on the whole, as they are moved to and fro along the bar, and the foremost one even made to pass over the end of it a little way, they always add to the magnetism already acquired. This consideration seems to enjoin setting F and G extremely near each other; for this seems to increase the sum, and to diminish the difference of their action. But it may be a question, Whether we gain more by strongly magnetising a very small part during the very short while that the magnets pass over it, or by acting on more of the bar at once, and continuing a weaker action for a longer while on this larger portion. Mr. Æpinus adds another consideration depending on his notion of the internal process; but we defer this to another opportunity. The safest direction seems to be, to place them at the distance which enables them to lift the greatest weight. They are then undoubtedly acting with the greatest effect.

Mr. Antheaume directs to place the touching magnets as in (Plate IV. fig. 6.) for a reason to be mentioned afterwards. Mr. Æpinus also recommends it for reasons founded on his own hypothesis. We must say, that, in our trials, we have found this method very sensibly superior, especially in the latter parts of the operation, when the resistance to farther impregnation becomes nearly a balance for the accumulating

power of the magnets; and we consider this as no inconsiderable argument for the justice of Mr. Æpinus's hypothesis.

The great advantage of this method is the regularity of the magnetism which it produces. We never find more than two poles; and when the bars are hard, and of uniform texture, the polarity is very little diffused, and seemingly confined to a very small space at the very extremities of the bar. This is indeed a prodigious advantage in point of strength. It is no less so in order to fit the magnets for experiments on the law of magnetic action; for the latitude which the diffused condition of the poles gives in the selection of the points from which the distances are to be computed, has hitherto hindered us from pronouncing on the law of magnetic action with the precision of which we think it fully susceptible. This method also is the only one by which we have been able to impregnate two bars joined end to end, considering them as one bar. We have sometimes (though very rarely) succeeded in this; so that when filings were strewed over them, the appearance could not be distinguished from a single bar.—*N. B.* Yet even in this case, in one experiment with two bars of six inches long, treated as one, when it could not be distinguished, either by the appearance of the filings, or by going round it very near with a compass needle, a very small compass needle discovered a neutral point, and a reversion of polarity similar to Plate III. fig 14. at F, shewing that it was really acting as two bars. Perhaps it must always be so; and this question is of considerable importance in the establishment of any theory of the internal process.

It deserves remark, that, in order to succeed in this attempt, a very considerable pressure is necessary. We were obliged to clean the ends of the bars very carefully, and to force the frame of bars and soft pieces of iron strongly together by wedges, in the manner of a form of types. We thought that wetting the ends of the bars with pure water aided the experiment; and we are *very certain* that oil not

only greatly obstructed it, but even sensibly impeded the common process. We had put a single drop of oil on a pair of bars which we were touching in the common Cantonian method, that the magnets might be more easily drawn along them; but we were surprised at finding that we could not give a strong impregnation. The oil undoubtedly prevents the close contact. We found the finest gold leaf produce the same effect in a great degree; as also talc, of which a square inch weighed $\frac{1}{24}$th of a grain. We do not infer any thing like obstruction to the passage of something material, but rather ascribe it to mere distance; although we are of opinion, that in the impregnation of two contiguous bars, so that the magnetism (whatever it is) is disposed *precisely* as in one bar, there *is* a material transference. But we shall speak of this in its due place.

It is not unworthy of remark, that we found bars to acquire more powerful magnetism when pretty well polished than when rough. But we also found, that bars considerably rough acquired the first degrees of it much more expeditiously than those which are smooth; although we never could bring them to that high degree of magnetism that the same bars acquired after they had been polished. We think it probable, that the tremors, occasioned by the rough and harsh surfaces of the hard steel, are the causes of this phenomenon.

Some more observations on this method of the double touch will be made afterwards, when we consider the hypothesis of Mr. Æpinus: and we conclude the present subject, by attempting to explain some puzzling appearances which frequently occur in making artificial magnets.

297. A bar touched by a very strong magnet has been said by Muschenbroek to be impaired by going over it with a weaker magnet. If it had been made as strong as possible, the weaker magnet when passed over it in the way practised by Muschenbroek, must first destroy part of this magnetism;

and having done so, it is unable to raise it anew to the same degree of vigour.

Yet (says Muschenbroek with surprise) a large bar of common iron has greatly improved the magnet. A very large piece of iron *must* do this (especially if shaped like a horse shoe, and applied with both heels), if the bar be not already at its maximum.

It was thought wonderful, that in the method of double touch, not only was the magnetism of the magnets employed not impaired, but, beginning with two magnets, whose power is almost insensible, and repeating the operations in the precise manner described by Mitchell or Canton, not only the bars intended to be made magnetical, but also the magnets employed, may be brought to their highest possible state of magnetism. This is in evident conformity to the general facts of induced magnetism, and affords the strongest proof that nothing is communicated in this operation, but that powers residing in the bars are excited, or brought into action. The manipulation merely *gives occasion* to this action, as a spark of fire kindles a city.

298. There still remain some circumstances of this method, as practised by Savery, Canton, and Antheaume, which are extremely curious and important.

Mr. Savery had observed a small bit of steel acquire very sensible magnetism by lying long in contact with the lower end of a great window bar. Telling this to a friend, he was, for the first time, informed, that this had been long observed, and that Dr. Gilbert had made some curious inferences from it. Mr. Savery wanted some magnets, and was at a distance from town. Reflecting, like a philosopher, on what he had heard and observed, he saw here a source of magnetism which he could increase, in the manner commonly practised in making magnets. He placed the bar AB (Plate IV. fig. 7.) to be magnetised between two great bars of common iron C and D, placing all the three in the magnetical direction. He took another bar EF,

and put two little pieces of iron, like the armour of a load stone, on its ends; and with those ends he rubbed the bar AB, rubbing the upper half of it with the end F, and the lower with the end E. The result of this was a very brisk magnetism in a few minutes, which, by various well devised alternations, he brought to its highest degree. His numerous experiments published in the Philosophical Transactions in 1746, contain much curious information, highly deserving the attention of the philosophers. Mr. Canton, proceeding on the same principle, that bars of iron, which have been long in a vertical position, acquire an efficient magnetism, begins his operations by placing his steel bar on the head of a kitchen poker, and rubs it with the lower end of a pair of kitchen tongs. Mr. Antheaume adheres more strictly to the inferences from the principle of terrestrial magnetism, and repeats precisely the previous disposition of things practised by Mr. Savery, placing his little steel bar AB (Plate IV. fig 8.) between two great bars C and D of common iron, and arranging the whole in the magnetic direction. Then, proceeding most judiciously on the same principle, he greatly improves the process, by employing two bars EF and GH for the touch, holding them about an inch apart, inclined about 15° to the bar AB. It is plain, that the lower end of each of these five bars is a north pole, and the upper end a south pole. Therefore the poles F and G concur in giving the proper magnetism to the portion FG of the steel bar which is between them; and by rubbing it with these poles up and down, overpassing each extremity about half an inch, he must soon give to the bar AB a regular magnetism; weak, perhaps, but to be afterwards increased in the Cantonian method, on a horizontal table. In this manner did Mr. Antheaume make magnets of very great strength in 1766. See his *Dissertation* already quoted

299. These observations naturally bring us to the PHYSICLOGIA NOVA DE MAGNETE ET CORPORIBUS MAGNETICIS

of Dr. Gilbert: a discovery which the sagacious Kepler classes among the greatest in the annals of science.

It could not be that a phenomenon so general, and so interesting and important as the natural polarity of magnetic bodies, would be long known without exciting curiosity about its cause. Accordingly the philosophers of the 16th century speculated much about it, and entertained a variety of opinion, if that can be called an opinion which can hardly be said to express a thought. We have in *Marsigli Ficino* a short notice of many of these opinions. Some maintained that the needle was directed by a certain point in the heavens, as if that were saying more than that it always pointed one way. Others, with more appearance of reasoning, ascribed the direction to vast magnetic rocks. But all this was without giving themselves the trouble of trying to ascertain what situation of such rocks would produce the direction that is observed. Fracastori was, if we mistake not, the first who thought this trouble at all necessary; and he observes very sensibly, that if those rocks are supposed to be in any place yet visited by navigators, and if they act as loadstones do, (a circumstance which he says must be admitted, if we attempt to explain,) the direction of the needle will be very different from what we know it to be. He therefore places them in the inaccessible polar regions, but not in the very pole. Norman, the discoverer of the dip of the mariner's needle, or of the true magnetic direction, was naturally led by his discovery to conceive the directing cause as placed in the earth; because the north point of the needle, in every part of Europe, points very far below the horizon. But although he calls the treatise in which he announces his discovery the *New Attractive*, he does not express himself as supposing the needle to be attracted by any point within the earth, but only that it is always directed to that point.

It is to Dr. Gilbert of Colchester that we owe the opinion now universally admitted, that magnetic polarity is a

part of the constitution of this globe. Norman had, not long before, discovered, that if a steel needle be very exactly balanced on a horizontal axis, like the beam of a common balance, so that it would retain any position given it, and if it be then touched with a magnet, and placed on its axis in the magnetic meridian, it is no longer in equilibrio, but (at London) the north point of it will dip 72 or 73 degrees below the horizon. He did not, however, publish his discovery till he had obtained information how it stood in other parts of the world. The differences in the variation in different places naturally suggested the necessity of this to him. Being a maker of mariners compasses, and teacher of navigation in London, he had the fairest opportunities that could be desired, by furnishing dipping needles to such of the navigators, his scholars, as he knew most able to give him good information. And the accounts which he received made his discovery, when announced to the world, a very complete thing; for the commanders of ships engaged in long voyages, and particularly to China, informed him that, in the vicinity of the equator, his dipping needles remained parallel to the horizon, but that in coming toward the north pole, the north end of the needle was depressed, and that the south end dipped in like manner at the Cape of Good Hope, and in the Indian Ocean; that the needle gradually approached the horizontal position as the ship approached the equator, but that in coming to the north of it at Batavia, the north point again dipped, and at Canton was several degrees below the horizon.

On these authorities, Norman boldly said that, in the equatoreal regions, the needle was horizontal, and that either end dipped regularly as it approached either pole; and that in the poles of the earth, the needle was perpendicular to the horizon. He therefore announced this as a discovery, not only singularly curious, but also of immense importance; for by means of a dipping needle the latitude of a ship at sea may be found without seeing the sun or stars.

Dr. Gilbert, comparing this position of the compass needle with the positions which he had observed small needles assume in his numerous experiments in relation to a magnet, as we have described at great length, was naturally led to the notion of the earth's being a great loadstone, or as containing one, and that this arranged the dipping, or, in general, the mariner's needle, in the same manner as he observed a great magnet arrange a small needle poised on its pivot. He therefore composed his *Physiologia Nova de Magnete*, et *de Tellure magno Magnete*; in which he notices so many points of resemblance to the directive power of a magnet, that the point seems no longer to admit of any doubt. Dr. Gilbert's theory may be thus expressed:

All the phenomena of natural magnetism are analogous to what we should observe, if the earth were a great magnet, having its poles near the poles of the earth's equator, the north pole not far from Baffin's Bay, and the south pole nearly in the opposite part of the globe. A dipping needle, under the influence of this great magnet, must arrange itself in a plane which passes through the poles of the magnet, the position of which plane is indicated (at least nearly) by the ordinary compass needle; and it will be inclined to the horizon so much the more as we recede from the equator of the great magnet.

This opinion of Dr. Gilbert was not less ingenious than important; and, if firmly established, it furnishes a complete theory of all the phenomena of magnetism. But observations were neither sufficiently numerous in the time of Dr. Gilbert, nor sufficiently accurate, to enable that great genius to assign the position of this great magnet, nor the laws of its action. The theory was chiefly founded on the phenomena of the dipping needle; phenomena which might have been unknown for ages, had the first notice of them fallen into any other hands than Norman's. They are not, like those of variation, which might be made by any sailor. They require for their exhibition a dipping needle, and the

attention to circumstances which can occur only to a mathematician. A dipping needle is to this day, notwithstanding all our improvements in the arts, one of the most delicate and difficult tasks that an instrument maker can take in hand, and a good one cannot be had for less than twenty guineas. We are confident that such as even Norman could make were far inferior to what are now made, and quite unfit for use at sea while the ship is under sail, although they may be tolerably exact for an observation of the dip in any port; and we presume that it was such observations only that Norman confided in. Our readers will readily conceive the difficulty of poising a needle with such a perfect coincidence of its centre of gravity and axis of motion, and perfect roundness of this axis, that it shall remain in any position that is given it. Add to this, that a grain of dust, invisible to the nicest eye, getting under one side of this axis, may be sufficient for making it assume another position. It must also be a difficult matter to preserve this delicate thing, so as that no change can happen to it Besides, all this must be performed on a piece of tempered steel which we are certain has no magnetism. Where can this be got, or what can insure us against magnetism? Nor is there less difficulty in making the observations without great risk of error. If the needle, moveable only in a vertical plane, be not set in the plane of a magnetic meridian, it will always dip too much*. At Lon-

* Let HZOF (Plate IV. fig. 23.) be the plane of a magnetic meridian, H n' O the plane of the horizon, and NS the position of the magnetic needle in any place, when it is at liberty to settle in the true magnetic direction. The angle HCN is the inclination or dip of the needle. Let Z n F be a vertical circle, in which a well constructed dipping needle can freely play up and down. This needle cannot place itself in the magnetic direction, because it can only move in a vertical plane. Its north point is impelled in the direction $n\,o$, and its south point in the direction $s\,p$, both of which are parallel to NS. By the laws of mechanical equilibrium, it cannot rest, except in such a position that the forces $n\,o$ and $s\,p$ are in a plane perpendicular to the plane Z n F. In any other position, there would be a force impelling the needle toward that side on which $n\,o$ makes an acute angle with the tangent $r\,n\,t$ of the vertical circle. There-

don, where the magnetic direction is inclined 73° to the horizon, if it be in a plane 20° from the magnetic meridian, it will stand almost perpendicular; for it is easy to see, by the mechanical resolution of forces, that it will take the position which brings it nearest to the true magnetic direction. This, we think, is confirmed by several of Norman's and other old observations of dip. They are much greater than they have been since found in the same places.

300. Mr. Daniel Bernoulli has given a very ingenious principle, by which we can make a dipping needle which will give a very accurate observation on shore; and being so easily executed, it deserves to be generally known. Let a dipping needle be made in the best manner that can be done by a workman of the place, and balanced with some care before impregnation, so that we may be certain that when touched it will take nearly the true dip. Touch it, and observe the dip. Destroy its magnetism, and then alter its balance in such a manner that, without any magnetism, it will arrange itself in the inclination of the observed dip. Now touch it again, giving it the same poles as before. It is plain that it will now approach exceedingly near indeed to the true dip, because its want of *perfect* equilibrium deranged it but a few degrees from the proper direction. If this second observation of the dip should differ several degrees from the first, by the inaccurate first formation of the needle, it will be proper to repeat the operation. Very rarely indeed will the third observation of the dip vary from the truth half a degree.

fore the spherical triangle N n F is right angled in n, and Cos. NF n : R = Tan. n F : Tan. NF, = Tan. HN, : Tan. n' n. Therefore

Tan. n' n = $\frac{\text{Tan. HN}}{\text{Cos. H } n'}$ = Tan. HN × Sec. H n'. Therefore, in any place, the real inclination of the magnetical direction to the horizon is different from what is pointed out by a dipping needle when it is in a plane which declines from the magnetic meridian; and the tangent of the observed dip of the needle exceeds that of the inclination of the magnetic direction in the proportion of radius to the cosine of the deviation HC n', or the proportion of the secant of this angle to the radius. If therefore the dipping needle play in a magnetic east and west circle, it will stand perpendicular to the horizon.

Mr. Bernoulli makes this simple contrivance answer the purpose of an universal instrument in the following ingenious manner. A very light brass graduated circle EFG (Plate IV. fig. 9.) is fixed to one side of the needle, concentric with its axis, and the whole is balanced as nicely as possible before impregnation. A very light index CD is then fitted on the axis, so as to turn rather stiffly on it. This will destroy the equilibrium of the needle. If the needle has been made with perfect accuracy, and perfectly balanced, the addition of this index would cause it always to settle with the index perpendicular to the horizon, whatever degree of the circle it may chance to point at. But as this is scarcely to be expected, set the index at various degrees of the circle, and note what inclination the unmagnetic needle takes for each place of the index, and record them all in a table. Suppose, for example, that when the index is at 50, the needle inclines 46° from the horizon. If in any place we observe that the needle (rendered magnetic by lying between two strong magnets), having the index at 50, inclines 46°, we may be certain that this is the dip at that place; for the needle is not deranged by the magnetism from the position which gravity alone would give it. As we generally know something of the dip that is to be expected in any place, we must set the index accordingly. If the needle does not shew the expected dip, alter the position of the index, and again observe the dip. See whether this second position of the index and this dip form a pair which is in the table. If they do, we have got the true dip. If not, we must try another position of the index. Noticing whether the agreement of this last pair be greater or less than that of the former pair, we learn whether to change the position of the index in the same direction as before, or in the opposite. The writer of this article has a dipping needle of this kind, made by a person totally unacquainted with the making of philosophical instruments. It has been used at Leith, at Cronstadt in Russia, at Scarborough, and at New York,

and the dip indicated by it did not in any single trial differ $1\frac{1}{2}$ degrees from other trials, or from the dip observed by the finest instruments. He tried it himself in Leith Roads, in a rough sea; and does not think it inferior, either in certainty or dispatch, to a needle of the most elaborate construction. It is worthy of its most ingenious author, and of the public notice, because it can be made for a moderate expence, and therefore may be the means of multiplying the observations of the dip, which are of immense consequence in the theory of magnetism, and for giving us an accurate knowledge of the magnetical constitution of this globe.

301. This knowledge is still very imperfect, owing to the want of a very numerous collection of observations of the dip. They are of more importance than those of the horizontal deviations from the meridian. All that we can say is, that the earth acts on the mariner's needle as a great loadstone would do. But we do not think that the appearances resemble the effects of what we would call a good loadstone, having the regular magnetism of two vigorous poles. The dips of the needle in various parts of the earth seem to be such as would result from the action of an extremely irregular loadstone, having its poles exceedingly diffused. The increase of the dip, as we recede from those places where the needle is horizontal, is too rapid to agree with the supposition of two poles of constipated magnetism, whether we suppose the magnetic action in the inverse simple or duplicate ratio of the distances, unless the great terrestrial magnet be of much smaller dimensions than what some other appearances oblige us to suppose. If there be four poles, as Dr. Halley imagined, it will be next to impossible to ascertain the positions of the dipping needle. It will be a tangent to one of the secondary magnetic curves, and these will be of a very intricate species. We cannot but consider the discovery of the magnetic constitution of this globe as a point of very great importance, both to the philosopher and to society. We have considered it with some care; but

hitherto we have not been able to form a systematic view of the appearances which gives us any satisfaction. The well informed reader is sensible, that the attempt by means of the horizontal or variation needle is extremely tedious in its application, and is very unlikely to succeed; at the same time it must be well understood. The two dissertations by Euler, in the 13th and 22d volumes of the Memoirs of the Royal Academy at Berlin, are most excellent performances, and give a true notion of the difficulty of the subject. Yet, even in these, a circumstance is overlooked, which, for any thing we know to the contrary, may have a very great effect. If the magnetic axis be far removed from the axis of revolution, as far, for example, as Mr. Churchman places it, the magnetic meridians will be (generally) much inclined to the horizon; and we shall err very far, if we suppose (as in Euler's calculus) that the dipping needle will arrange itself in the vertical plane, passing through the direction of the horizontal or variation needle; or if we imagine that the poles of the great magnet are in that plane. We even presume to think that Mr. Euler's assumption of the place of his fictitious poles (namely where the needle is vertical), in order to obtain a manageable calculus, is erroneous. The introduction of this circumstance of inclination of the magnetic meridians to the horizon, complicates the calculation to such a degree as to make it almost unmanageable, except in some selected situations. Fortunately, they are important ones for ascertaining the places of the poles. But the investigation by the positions of the dipping needle is incomparably more simple, and more likely to give us a knowledge of a multiplicity of poles. The consideration of the magnetic curves (in the sense used in the present article), teaches us that we are not to imagine the poles immediately under those parts of the surface where the needle stands perpendicular to the horizon, nor the magnetic equator to be in those places where the needle is horizontal; a notion commonly and plausibly entertained. Unfortunately our most numerous observa-

tions of the dip are not in places where they are the most instructive. A series should be obtained, extending from New Zealand northward, across the Pacific Ocean to Cape Fairweather on the west coast of North America, and continued through that part of the continent. Another series should extend from the Cape of Good Hope, up along the west coast of Africa to the tropic of Capricorn; from thence across the interior of Africa (where it would be of great importance to mark the place of its horizontality) through Sicily, Italy, Dalmatia, the east of Germany, the Gulph of Bothnia, Lapland, and the west point of Greenland. This would be nearly a plane passing through the probable situations of the poles. Another series should be made at right angles to this, forming a small circle, crossing the other near Cape Fairweather. This would pass near Japan, through Borneo, and the west end of New Holland; also near Mexico, and a few degrees west of Easter Island. In this place, and at Borneo, the inclination of the magnetic plane to the horizon would be considerable, but we cannot find this out. It may, however, be discovered in other points of this circle, where the dip is considerable. We have not room in this short account to illustrate the advantages derived from these serieses; but the reflecting reader will be very sensible of them, if he only supposes the great magnet to be accompanied by its magnetic curves, to which the needle is always a tangent. He will then see that the first series from New Zealand to Cape Fairweather, and the second from Cape Fairweather round the other side of the globe, being in one plane, and at very different distances from the magnetic axis, must contain very instructive positions of the needle. But we still confess, that when we compare the dips already known with the variations, they appear so irreconcileable with the results of an uniform regular magnetism, that we despair of success. Every thing seems to indicate a multiplicity of poles, or, what is still more adverse to all calculation, an irregular magnetism with very diffused polarity.

Much instruction may surely be expected from the observations of the Russian academicians and their eleves, who are employed in surveying that vast empire; yet we do not meet with a single observation of the dip of the needle in all the by-gone publications of that academy, nor indeed are there many of the variation.

302. For want of such information, philosophers are extremely divided in their opinions of the situation of the magnetic poles of this globe. Professor Krufft, in the 17th volume of the Petersburgh Commentaries, places the north pole in lat. 70° N. and long. 23° W. from London; and the south pole in lat. 50° S. and long. 92° E.

Wilcke of Stockholm, in his indication chart (*Swed. Mem.* tom. xxx. p. 218.), places the north pole in N. Lat. 75°, near Baffin's Bay, in the longitude of California. The south pole is in the Pacific Ocean, in lat. 70° S.

Churchman places the north pole in lat. 59° N. and long. 135° W. a little way inland from Cape Fairweather; and the south pole in lat. 59° S. long. 165° E. due south from New Zealand.

A planisphere by the Academy of Sciences at Paris for 1786, places the magnetic equator so as to intersect the earth's equator in long. 75°, and 155° from Ferro Canary Island, with an inclination of 12 degrees nearly, making it a great circle very nearly. But we are not informed on what authority this is done; and it does not accord with many observations of the dip which we have collected from the voyages of several British navigators, and from some voyages between Stockholm and Canton. Mr. Churchman has given a sketch of a planisphere with lines, which may be called parallels of the dip. Those parts of each parallel that have been ascertained by observation are marked by dots, so that we can judge of his authority for the whole construction. It is but a sketch, but gives more synoptical information than any thing yet published. The magnetic equator cuts the earth's equator in long. 15°, and 195° E.

from Greenwich, in an angle of nearly 17 degrees. The circles of magnetic inclination are not parallel, being considerably nearer to each other on the short meridian than on its opposite. This circumstance, being founded on observation, is one of the strongest arguments for the existence of a magnet of tolerable regularity, as the cause of all the positions of the compass needle; for such *must* be the positions of the circles of equal dip, if the axis of this magnet is far removed from the axis of rotation, and does not intersect it.

The celebrated astronomer Tobias Mayer of Gottingen, proposed the following hypothesis, by which the direction of the mariner's needle in all parts of the earth may be determined. He supposes that the earth contains a very powerful magnet of inconsiderable dimensions, which arranges the needle according to the known laws of magnetism. The centre of this magnet was distant from the centre of the earth about 480 English miles in 1756, and a line joining these centres intersected the earth's surface in a point situated in 17° N. Lat. and 183° E. Long. from London. The axis of the magnet is perpendicular to this line, and the plane in which it lies is inclined about 11° to the plane of the meridian, the north end of the axis lying on the east side of that meridian. From these data, it will be found that the axis of this magnet cuts the surface of the earth about the middle of the eastern shore of Baffin's Bay, and in another point about 800 miles S. S. W. of the southern point of New Zealand. Professor Lichtenberg of Gottingen, who gives this extract from the manuscript, says, that the hypothesis is accompanied by a considerable list of variations and dips calculated by it, and compared with observations, and that the agreement is very remarkable. He gives indeed a dozen instances in very different regions of the earth. But we suspect that there is some error or defect in the data given by him, because the annual changes, which he also gives, are such as are inconsistent with the data, and

even with each other. He says, that the distance from the centre increases about four miles annually, and that *thence* arises an annual diminution of 8 minutes in the latitude and 14 in the longtitude of that point where the straight line joining the centres meets the surface. It can have no such consequence. He says also, that the above mentioned inclination of the planes increases 8 minutes annually. The compound force of the magnet is said to be as the square root of the distance inversely. We are at a loss to understand the meaning of this circumstance; because Mayer's hypothesis concerning the law of magnetic action is exceedingly different, as related by Mr. Lichtenberg from the same manuscript. But it was our duty to communicate this notice, though imperfect, of the speculations of this celebrated mathematician. See *Exliben's Elem. of Nat. Phil.* published by Lichtenberg 1784. p. 645.

303. Now, if the situation of the poles be any thing near the average or medium of these determinations, and if we form all our notions by analogy, comparing the positions of the compass needle in relation to the great terrestrial magnet, with the positions assumed by a small needle in the neighbourhood of a magnet, we must conclude, that the magnetical constitution of this globe has little or no reference to its regular external form. The axis of the magnet is very far removed from that of the globe (at least 1500 miles), and is not nearly parallel to it, nor in the same plane. It required the sagacity and the skill of a Euler to subject such anomalous magnetism to any rules of computation; and every person qualified to judge of the subject must allow his dissertation in the 13th volume of the Berlin Memoirs to be a work of wonderful research. It is a very agreeable thing to see such a conformity between the lines which express the regular magnetism of Euler's dissertation, and the lines drawn by Dr. Halley from observation, and which appeared to himself so capricious, that he despaired (notwithstanding his consummate skill in geometry) of their ever being reduced to a mathematical and precise system.

304. Without detracting from the merit of Dr. Gilbert, we may presume to say that his notion of the earth's being a great magnet was not, in his mind, more than a sagacious conjecture, formed from a very general and even vague comparison. Yet the comparison was sufficiently good to give him great confidence in his opinion that the action of this great magnet, in perfect conformity to what we observe in our experiments with magnets, is the source of all the magnetism that we obseve. If there was nothing else in proof of the justness of his theory, it is abundantly proved by the beautiful experiment of Mr. Henshaw, mentioned in the article VARIATION, *Encycl.* p. 621. col. 2. An iron bar held nearly upright, attracts the south end of a compass needle with its lower end; and if that end of the bar be kept in its place, and the bar turned round till it becomes the upper end, the south point of the needle immediately turns away from it, and the north end is now attracted. This experiment may be perfectly imitated with artificial magnetism.

Having supported a large magnet SAN (Plate IV. fig. 10.) so that its ends are detached from surrounding bodies, place a small needle B (poised on its pivot) about three inches below the north pole N of the magnet, and in such a situation that its polarity to the magnet may be very weak. Take now a small piece of common iron, and hold it in the position represented at C. Its lower end becomes a north pole, attracting the south pole of the needle. Keeping this in its place, turn round the piece of iron into the position D; the south pole of B will now avoid it, and the north pole will be attracted. We directed the needle to be so placed, that its polarity in relation to the magnet, may be weak. If it be strong, it may act on the end of C or D like a magnet, and counteract the magnetism induced on C or D by vicinity to A.

An anonymous writer in the Philosophical Transactions, No. 177. Vol. XV. relates several observations made during a voyage to the East Indies, which are quite conformable to

this. A few leagues northwest from the island Ascension, the south point of the compass needle hardly shewed any tendency to or from the lower end of an iron bar. It seemed rather to avoid the upper end; it was not in the least affected by the middle of the bar; but when the bar was laid horizontal, in the magnetic direction, its two ends affected the dissimilar ends of the compass needle very strongly; but when horizontal, and lying at right angles to the magnetic direction, its polarity was altogether indifferent.

As the other phenomena of induced artificial magnetism have the same resemblance to the phenomena of natural magnetism, a bar which has remained long in the vicinity of a magnet acquires magnetism (permanent) in the same way, and modified by the same circumstances, as in natural magnetism. Hammering a bit of common iron in the immediate vicinity of a magnet, gives it very good magnetism. Exposing a red hot bar to cool in the neighbourhood of a magnet has the same effect. Also quenching it suddenly has the same effect. Quenching a small red hot steel bar between two magnets, was found by us to communicate a much stronger magnetism than we could give it by any other method. Its form indeed was very unfavourable for the ordinary method of touching; for it consisted of two little spheres connected by a slender rod, and could scarcely be impregnated in any other way than by placing it for a very long while between magnets. In all these experiments, the polarity acquired is precisely similar to that acquired by the same treatment in relation to this supposed great terrestrial magnet. In short, in whatever manner we pursue this analogy in our experiments, we find the resemblance most perfect in the phenomena.

We cannot but think, therefore, that this new physiology of the magnet by Dr. Gilbert is well established; and we think ourselves authorised to assume it as a proposition fully demonstrated, that the earth is a great magnet, or contains a great magnet, the agency of which produces the di-

rection of the magnetic needle, and all the magnetism which iron acquires by long continuance in a proper position. It is this which made us say, in the beginning of this article, that attraction and polarity were not confined to magnets, but were properties belonging to all iron in its metallic state. We now see the reason why any piece of iron brought very near to another piece will attract it—both become magnetical, in consequence of the agency of the great magnet; and their magnetism is so disposed, that their mutual attractions exceed their repulsions. Also, why an iron rod, placed nearly in the magnetical direction, will finally arrange itself in that direction. Also, why the terrestrial polarity of common iron is indifferent, and either end of the rod will settle in the north, if it have nearly that position at first. The magnetism induced by mere momentary position is so feeble as to yield to any artificial magnetism. As a moment was sufficient for imparting it, a moment suffices for destroying it; and another moment will impart the opposite magnetism. But artificial magnetism requires more force for its production, and some of it remains when the producing cause is removed, and it does not yield at once to the contrary magnetism. That there is no farther difference appears from this, that long continued position gives determined and permanent magnetism, and that it is destroyed by an equally long continuance in the contrary position. It seems to be very generally true, that a magnet will carry more by its north than by its south pole. It should be so in this part of the world, because the terrestrial magnetism induced on the iron conspires with the magnetism induced by the north pole of a magnet, but counteracts the magnetism induced by the south pole.

The propriety of Mr. Savery's, Mr. Canton's, and Mr. Antheaume's processes for beginning the impregnation of hard steel bars is now plain, and the superior effect of the two great bars of common iron in the proposed method of Mr. Antheaume. We cannot but take this opportunity of

paying the proper tribute of praise to the ingenuity of Mr. Savery. Every circumstance of his process was selected in consequence of an accurate conception of magnetism, and the combination of this science with Dr. Gilbert's theory. His process is the same with Antheaume's in every respect, except the circumstance of the double touch borrowed from Mitchell and Canton. These observations do not detract from the discernment of Mitchell and Canton, who saw in those experiments what had escaped the attention of hundreds of readers.

305. But there occurs an objection to this theory of Dr. Gilbert, which was urged against it with great force. We observe no tendency in the magnet or compass needle toward this supposed magnet. An iron or steel bar is not found to increase its tendency downwards, that is, is not sensibly heavier, when its south pole is uppermost in this part of the world. A needle set afloat on a piece of cork arranges itself quickly in the proper direction; but if continued ever so long afloat, it has never been observed to approach the north side of the vessel. This is quite unlike what we observe in the mutual actions of magnets, or the action of magnets on iron. This objection appears to have given Dr. Gilbert some concern; and he mentions many experiments which have been tried on purpose to discover some magnetical tendency. He gets rid of it as well as he can, by saying, that the directive power of a magnet extends much farther than its attractive power. He confirms this by several experiments. But Dr. Gilbert had not studied the simultaneous actions of the four poles, nor explained, by the principles of compound motion, how these produced all the possible positions of the needle. Indeed, the composition of mechanical forces was by no means familiar with philosophers at the end of the 16th century. We see it now very distinctly. The polarity of the needle, or the force with which it turns itself into the magnetical position, depends on the difference between the *sums* of the actions of

each pole of the magnet on both the poles of the needle; whereas its tendency towards the magnet depends on the difference of the *differences* of those actions (see § 259, 262.) The first may thus be very great when the other is almost insensible. We see, that coarse iron filings heap about the magnet very fast, and that very fine filings approach it very slowly. Now, the largest magnet that we can employ, when compared with the great magnet in the earth, is but as a particle of the finest filings that can be conceived. This surely diminishes exceedingly, if it does not entirely annihilate the objection: but as we have heard it urged by many as an improbable thing, that a long magnet, kept afloat for many months (which has been done) shall not shew the *smallest* tendency towards the pole of the terrestrial magnet, we think it deserves to be considered with accuracy, and the question decided in a way which will admit of no doubt.

306. Let the very small magnet C (Plate IV. fig. 11.) be placed near a great magnet A, and then near a smaller magnet B, in such a manner that its polarity to both shall be the same; and then let us determine the proportion between the attractions of A and B for the small magnet C.

This will evidently depend on the law of magnetic action. For greater simplicity of investigation, we shall content ourselves with supposing the action to be inversely as the distance.

Let AN, = AS, = a; BN = b; C n = c, AC = d, BC = δ; and let the absolute force of A be to that of B at the same distance as m to 1.

The magnetic action being supposed proportional to $\frac{1}{d}$, we have,

1. Action of AN on C $s = \frac{m}{d - a - c}$.

2 ——— AN on C $n = -\frac{m}{d - a + c}$

3. ——— AS on C $s = -\frac{m}{d + a - c}$

4. ——— AS on C $n = \frac{m}{d + a + c}$.

5. The whole action $= \frac{8\,m\,a\,c\,d}{\sqrt{d^2 - \overline{a + c}^2} \times \sqrt{d^2 - \overline{a - c}^2}}$.

6. If c be very small in comparison with a or b, the whole action of A is very nearly $= \frac{8\,m\,a\,c\,d}{d^2 - a^2}$.

7. And the tendency of C to B is, in like manner, $= \frac{8\,b\,c\,\delta}{\delta^2 - b^2}$.

The directive powers of A and B are at their maximum state when C is placed with its axis at right angles to the lines AC or BC. In which case we have,

8. The directive power of A $= \frac{4\,m\,a}{d^2 - a^2}$.

9. The directive power of B $= \frac{4\,b}{\delta^2 - b^2}$.

When these directive powers are made equal, by placing C at the proper distances from A and B, we have,

10. $4\,m\,a : 4\,b$, or $m\,a : b = d^2 - a^2 : \delta^2 - b^2$

And $m\,a\,\delta^2 - m\,a\,b^2 = b\,d^2 - b\,a^2$

$m\,a\,\delta^2 = b\,(d^2 - a^2) + m\,a\,b^2$.

11. $\delta^2 = \frac{b}{m\,a}(d^2 - a^2) + b^2$.

12. $\delta = \sqrt{\frac{b}{m\,a}\,(d^2 - a^2) + b^2}$.

Let the attractions of A and B for the very small magnet C, when its polarity to both is the same, be expressed by the symbols α and β. We have

$\alpha : \beta = \frac{8\,m\,a\,c\,d}{(d^2 - a^2)^2} : \frac{8\,b\,c\,\delta}{(\delta^2 - b^2)^2}$, which, by No. 10, is $= \frac{8\,(d^2 - a^2)\,c\,d}{(d^2 - a^2)^2} : \frac{8\,(\delta^2 - b^2)\,c\,\delta}{(\delta^2 - b^2)^2} = \frac{d}{d^2 - a^2} : \frac{\delta}{\delta^2 - b^2}$, $= b\,d : m\,a\,\delta$; that is,

13. Attr^{n} of A : attr^{n} of B $= b\,d : m\,a\,\delta$

As an example of this comparison, let us suppose the great terrestrial magnet to be a thousand times larger and stronger than the magnet whose attraction we are comparing with that of terrestrial magnetism. Let us also suppose the distance from the pole of the great magnet to be small, so that its attraction may be considerable. Let us make $d = 1200$, a being $= 1000$, and $b = 1$. These are all very reasonable suppositions. Substituting these values in the formula, we have attr^{n} of A : attr^{n} of B $= 1 : 1000$ very nearly; and therefore when the needle, when placed near a magnet, vibrates by its polarity as fast as it does by natural magnetism, its tendency toward that magnet must be altogether insensible; for the disproportion is incomparably greater than that of 1 to 1000, in the largest magnets with which we can make experiments. Observe also, that we have taken the case where the attractions are the strongest, viz. when the magnet C is placed in the axis of A or B. In the oblique positions, tangents to the magnetic curves, the attractions are smaller, almost in any ratio.

We took the inverse ratio of the distances for the law of action, only because the analysis was very simple. It is very evident, that the disproportion will be still more remarkable if the action be inversely as the square of the distance.

The objection therefore to the origin of the polarity of the compass needle, and of all other magnets, namely, the action of a great magnet contained in the earth, appears plainly to be of no force. We rather think that the want of all sensible attraction, where there is a brisk polarity, is a proof of the justness of the conjecture; for if the compass needle were arranged by the action of magnetic rocks, or even extensive strata, near the surface of the earth, the attractions would bear a greater proportion to the polarities. We have even observed this. A considerable mass of magnetic stratum was found to derange the needle of a surveyor's theodolite at a considerable distance all around (about 140 yards). The writer placed the needle on a thin lath, which just floated it

on water in a large wooden dish, and set it in a place where it was drawn about 15 degrees from the magnetic meridian. It was left in that situation a whole night, well defended from the wind by a board laid on the dish. Next morning it was found applied to that side of the dish which was nearest to the disturbing rocks. It had moved about six inches. This was repeated three times, and each time it moved in the same direction (nearly), which differed considerably from the direction of the needle itself.

It is now plain that we may, with confidence, assume Dr. Gilbert's theory of terrestrial magnetism as sufficiently established. And, since we must certainly call that the north pole of the great magnet which is situated in the northern parts of the earth, and since those poles of magnets which attract each other have opposite polarities, we must say, that what we call the north pole of a mariner's needle, or of any other magnet, has the southern polarity.

307. We may now venture to go farther with Dr. Gilbert, and to say that *all* the magnetism which we observe, whether in nature or art, is either the immediate or the remote effect of the action of the great magnet. As soft bars soon acquire a transient magnetism; as hard bars, after long exposure, acquire a sensible and permanent magnetism—we must infer, that ores of iron, which are in a state fit for impregnation, must acquire a sensible and permanent magnetism, by continuing, for a series of ages, in the bowels of the earth. And thus the magnetism of loadstones, which, till the discovery of the natural magnetism acquired by position, were the sources of all our magnetical phenomena, is now proved to be a necessary consequence of the existence and agency of a great magnet contained in the bowels of the earth.

308. It seems to result from this theory, that, in these northern parts of the world, that part of every natural loadstone that is at the extremity of the line drawn through the stone in the magnetic direction should be its pole; and that

the loadstone, when properly poised, should of itself assume the very position which it had in the mine. Dr. Gilbert complains of the inattention of miners *(rude hominum genus, lucro potius quam physicæ consulentes)* to this important circumstance. Once, however, he had the good fortune to be advertised of a great magnetic mass lying in its matrix. He repaired quickly to the mine, examined it, and marked its points which were in the extremities of the magnetic line. When it was detached from its matrix, he had the pleasure of finding its poles in the very places he expected. The loadstone was of considerable size, weighing about 20 pounds. —Mr. Wilcke gives in the Swedish Commentaries several instances of the same kind.

But should this always be the case? By no means. There are many circumstances which may give the magnetism of a loadstone a very different direction. We have found, that simple juxtaposition to a magnet will sometimes give a succession of poles to a long bar of hard steel. The same thing may happen to an extensive vein of magnetisable matter. The loadstone taken out of this vein may have been placed like that of a soft bar placed in the magnetic line, if lying in one part of the vein; if taken from another part of, its polarity may be the very reverse; and in another part it may have no magnetism, although completely fitted for acquiring it. It may have its poles placed in a direction different from all these, in consequence of the vicinity of a greater loadstone. As loadstones possessed of vigorous magnetism are always found only in small pieces, and in pieces of various sizes and force, we must expect every position of their poles. The only thing that we can expect by theory is, that adjoining loadstones will have their friendly poles turned toward each other, and a general prevalence of or tendency to a polarity symmetrical with that of the earth. The reader will find some more observations to this purpose in the article VARIATION, in the APPENDIX to this dissertation, as also in Gilbert's treatise, B. III. c. 2. p. 121.

Nor should all strata or masses of iron ore be magnetical. We know that none are susceptible of induced magnetism, but such as are, to a certain degree, in the metallic state. Such ores are not abundant. Nay, even all of such strata do not necessarily acquire magnetism by the action of the great magnet. If their principal dimensions lie nearly perpendicular to the magnetic direction, they will not acquire any sensible quantity. A stratum in this country, rising about 17 degrees to the N. N. W. will scarcely acquire magnetism. It may also happen, that the influence of the great magnet is counteracted by that of some extensive stratum inaccessible to man, by reason of its great depth.

309. Thus we see that all the appearances of the original magnetism of loadstones are perfectly consistent with the notion that they are effects of one general cosmical cause, the action of the great magnet contained in the earth, and that there is no occasion to suppose this great magnet to differ, in its constitution or manner of action, from the small masses of similar matter called loadstone. The only difficulty that presents itself is the great superiority of magnetic force observable in some loadstones over other masses of ores circumjacent, which are not distinguishable by us by any other circumstance. We acknowledge ourselves unable to solve this difficulty ; for the magnetism of such pieces is sometimes incomparably stronger than what a bar of iron acquires by position ; yet this bar is much more susceptible than the ores which are fit for becoming loadstones Perhaps there is some chemical change which obtains gradually in certain masses, which aids the impregnation, in the same way that we know that being red hot destroys all magnetism, whether in a metal bar or in an ore. This seems to be confirmed by what we see in some old iron stanchions, which acquire the strongest magnetism in those parts of their substance which are combining themselves with ingredients floating in the atmosphere. That part which is cased in the stone, and exfoliates and splits with rust, being con-

verted into something like what is called finery-cinder, becomes highly and permanently magnetic. Such peculiarities as these, operating for ages, may allow a degree of magnetical impregnation (in whatever this may consist) to take place, to which we can see no resemblance in our experiments. It would be worth while to place iron wires in a tube in the magnetic direction, which could be kept of a proper red heat, while it is converted into æthiops by steam. It is not unlikely that it would acquire a sensible and permanent magnetism in this way. It may be, that the little atoms, as they arrange themselves in a sort of crystalline or symmetrical form, may also arrange so as to favour magnetism. Were this tried in the vicinity of a strong magnet, the effect might be more remarkable and precise. Perhaps, too, while iron is precipitated in a metallic form from its solutions by another metal, something of the same kind may happen. We know, that proper ores of iron, exposed to cementation in a low red heat, in the magnetic direction, become magnetic.

310. Notice has been taken in the APPENDIX, on the variation of the Compass, of the attempts of ingenious men to explain the change which is observed in all parts of the globe, on the direction of the mariner's needle, the gradual change of the variation. The hypothesis of Dr. Halley, that the globe which we inhabit is hollow, and incloses a magnetic nucleus, moving round another axis, is not inconsistent with any natural law, if he did not suppose the interval filled up with some fluid The action of the nucleus and shell on the intervening fluid, would gradually bring the two to one common motion of rotation, as may be inferred from the reasonings employed by Newton in his remarks on the Cartesian vortices.

Leaving out this circumstance, there is only another cause which can affect, and must affect the rotation of both, namely, the mutual action of the magnetic nucleus, and the masses of magnetic matter in the shell. If the axis of rotation of this nucleus be different from the line joining its

magnetic poles, these poles will have a motion relative to the shell; and this motion may easily be conceived such as will produce the changes of magnetic direction which we observe. It may even produce a motion of the northern magnetic pole in one direction, and of the southern pole in the opposite direction, and this with the appearance of different periods of rotation, as supposed by Mr. Churchman. We may here observe, by the way, that the change of magnetic direction in this country is not nearly so great as is commonly imagined. The horizontal needle has shifted its position about 35° at London since 1585; but the point of the dipping needle has not changed 10°. We may also observe, that when the pole of the central magnet changes its place, the magnetism of an extensive stratum, influenced by it, may so alter its disposition, as to change the position of the compass needle in the opposite direction to that of the change which the central magnet alone would induce on it.

But as motions have not yet been assigned to this nucleus, which quadrate with the observed positions of the needle, and as the very existence of it is hypothetical, it may not be amiss to examine, whether such a change of variation may not be explained by what we know of the laws of magnetism, and of the internal constitution of this earth?

1. It is pretty certain, that the veins in which loadstones are found are not parts of the great magnet. This appears from their having two poles while in the mine, and also from the very small depth to which man has been able to penetrate. When we compare the positions of the dipping needle with those of a small needle near a magnet, we must infer, that the poles are very far below the surface.

Yet we know, that there are magnetisable strata of very great extent occupying a very considerable portion of the external covering. Though their bulk and absolute power may be small, when compared with those of the great magnet, yet their greater vicinity to the needles on which observations are made, may give them a very sensible influence. In this way may a great deal of the observed irregularities

of the positions of the needle be accounted for. In the Lagoon at Teneriffe, *Feuillé* observed the variation 13° 36′ west in 1724, while at the head of the island it was only 5°. The dip at the Lagoon was 63° 30′, greatly surpassing what was observed in the neighbourhood. Muller found, in the mountains of Bohemia, great and desultory differences of declination, amounting sometimes to 50°. At Mantua, the variation in 1758 was 12° ; while at Bononia and Brixia it was nearly 18°. Great irregularities were observed by Goëte in the Gulph of Finland, especially near the island of Sussari, among some rocks : on one of these, the needle shewed no polarity. Captain Cook and Captain Phipps observed differences of 10°, extending to a considerable distance, on the west coasts of North America. In the neighbourhood of the island Elba in the Mediterranean, the position of the needle is greatly affected by the iron strata, in which that island so much abounds. In this country, there are also observed small deviations, which extend over considerable tracts of country, indicating a great extent of strata that are weakly magnetic. Since such strata receive their magnetism by induction, in a manner similar to a bar of hard steel, and since we know that this receives it gradually, it may very probably happen, that a long series of years may elapse before the magnetism attains its ultimate disposition.

Here, then, is a necessary change of the magnetic direction; and although it may be very different in different places, according to the disposition and the power of those strata, there must be a general vergency of it one way.

2. It is well known that all metals, and particularly iron, are in a progress of continual production and demetallization. The veins of metals, and more particularly those of iron, are evidently of posterior date to that of the rocks in which they are lodged. Chemistry teaches us, by the very nature of the substances which compose them, that they are in a state of continual change. This is another cause of change in the magnetic direction. Nay, we know that some of them have suddenly changed their situation by earth-

quakes and volcanoes. Some of the streams of lava from Vesuvius and Ætna abound in iron. This has greatly changed its situation; and if the strata from which it proceeded were magnetical, the needle in its neighbourhood must be affected. Nay, subterranean heat alone will effect a change, by changing the magnetism of the strata. Mr. Lievog, royal astronomer at Bessestedt in Iceland, writes, that the great eruption from Hecla in 1783, changed the direction of the needle nine degrees in the immediate neighbourhood. This change was produced at a mile's distance from the frozen lava; and it diminished to two degrees at the distance of $2\frac{1}{2}$ miles. He could not approach any nearer, on account of the heat still remaining in the lava, after an interval of 14 months.

All these causes of change in the direction of the mariner's needle must be partial and irregular. But there is another cause which is cosmical and universal. Dr. Halley's supposition of four poles, or, at least, the supposition of irregular and diffused poles, seems the only thing that will agree with the observations of declination. We know that all magnetism of this kind (that is, disposed in this manner) has a natural tendency to change. The two northern poles may have the same or opposite polarities. If they are the same, their action on each other tends to diminish the general magnetism, and to cause the centre of effort to approach the centre of the magnet. If they have opposite polarities, the contrary effect will be produced. The general magnetism of each will increase, and the pole (or its centre of effort) will approach to the surface. In either of these cases, the compound magnetism of the whole may change exceedingly, by a change by no means considerable in the magnetism of each pair of poles. It is difficult to subject this to calculation; but the reader may have very convincing proof of it, by taking a strong and a weaker magnet of the same length, and one of them, at least, of steel not harder than spring temper. Lay them across each other like an acute letter

X; and then place a compass needle, so that its plane of rotation may be perpendicular to the plane of the X. Note exactly the position in which the needle settles. In a few minutes after, it will be found to change considerably, although no remarkable change has yet happened to the magnets themselves.

311. We flatter ourselves, that our readers will grant that the preceding pages contain what may justly be called a theory of magnetism, in as much as we have been able to include every phenomenon in one general fact, the induction of magnetism; and have given such a description of that fact and its modifications, that we can accurately predict what will be the appearances of magnets and iron put into any desired situation with respect to each other.

But it is not easy to satisfy human curiosity. Men have even investigated, or sought for causes of the perseverance of matter in its present condition. We have not been contented with Newton's theory of the celestial motions, and have sought for the cause of that mutual tendency which he called gravitation, and of which all the motions are particular instances.

Philosophers have been no less inquisitive after what may be the cause of that mutual attraction of the dissimilar poles, and the repulsion of the similar poles, and that faculty of mutual impregnation, or excitement, which so remarkably distinguish iron, in its various states, from all other substances. The action of bodies on each other at a distance, has appeared to them an absurdity, and all have had recourse to some material intermedium. The phenomenon of the arrangement of iron filings is extremely curious, and naturally engages the attention. It is hardly possible to look at it without the thought arising in the mind of a stream issuing from one pole of the magnet, moving round it, entering by the other pole, and again issuing from the former outlet. Accordingly, this notion has been entertained from the ear-

liest times, and different speculatists have had different ways of conceiving how this stream operated the effects which we observe.

The simplest and most obvious was just to make it act like any other stream of fluid matter, by impulsion. Impulsion is the thing aimed at by all the speculatists. They have a notion, that we conceive this way of communicating motion with intuitive clearness, and that a thing is fully explained when it can be shewn that it is a case of impulsion. We have considered the authority of these explanations in the article IMPULSION, in the *Suppl.* to the *Encycl. Brit.* and need not repeat our reasons for refusing it any pre-eminence. But even when we have shewn the phenomena to be cases of impulsion by such a stream, the greatest difficulty, the most curious and the most embarrassing, is to ascertain the sources of this impulsive motion of the fluid—How, and from what cause does it begin? What forces bend it in curves round the magnet? Those philosophers, whose principle obliges them to explain gravitation also by impulse, must have another stream to impel this into its curves. Acting by impulsion, this magnetic stream must lose a quantity of motion equal to what it communicates. What is to restore this? What directs it in a particular course through the magnet? And what is it that can totally alter that course —in a moment—in all the phenomena of induced magnetism? How does it impel? Lucretius, either of himself, or speaking after the Greek philosophers, makes it impel, not the iron, but the surrounding air, sweeping it out of the way; and thus giving occasion for the surrounding air to rush around the magnet, and to hurry the bits of iron toward it. There is, perhaps, more ingenious refinement in this thought than in any of the impulsive theories adopted since his day by Des Cartes, Euler, and other great philosophers: But it is sagaciously remarked by D. Gregory, in his MS. notes on Newton, that this theory of Lucretius falls to the ground; because the experiments succeed just as well under water as in the air. As to the explanations, or de-

scriptions, of the canals and their dock gates, opening in one direction, and shutting in the other, constructions that are changed in an instant in a bar of iron, by changing the position of the magnet, we only wonder that men, who have a reputation to lose, should ever hazard such crude and unmechanical dreams before the public eye. The mind of man cannot conceive the possibility of their formation; and if they are really formed, the effects should be the very opposite of those that are observed: the stream should move those bodies least which afford ready channels for its passage. If a rag of iron filings be arranged by the impulsion of such a stream, it should be carried along by it; and if it is *impelled toward* one end of the magnet, it should be impelled *from* the other end. Since we now know, that each particle of filings is a momentary magnet, we must allow a similar stream whirling round each. Is that an explanation, which exceeds all power of conception?

But has it ever been shewn, that there is any impulsion at all in these phenomena? Where is the impelling substance? The only argument ever offered for its existence is, that we are resolved that the phenomena of magnetism shall be produced by impulsion, and the arrangement of iron filings looks somewhat like a stream. But enough of this. We trust that we have shewn the way in which this arrangement obtains in the clearest manner. Every particle becomes magnetic by induction. This is a fact, which sets all reasoning at defiance. The polarity of each rag is so disposed that their adjoining ends turn to each other. This is another uncontrovertible fact. And these two facts explain the whole. The arrangement of iron filings, therefore, is a secondary fact, depending on principles more general; and therefore cannot, consistently with just logic, be assumed as the foundation of a theory.

Had magnetism exhibited no phenomena besides the attraction and repulsion of magnets, it is likely that we should not have proceeded very far in our theories, and would have contented ourselves with reducing these phenomena to their

most general laws. But the communication of magnetism seems a great mystery. The simple approach of a magnet communicates these powers to a piece of iron; and this without any diminution of its own powers. On the contrary, beginning with magnets which have hardly any sensible power, we can, by a proper alternation of the manipulations, communicate the strongest magnetism to as many hard steel bars as we please; and the original magnets shall be brought to their highest degree of magnetism. We have no notion of powers or faculties, but as qualities of some substances in which they are inherent. Yet here is no appearance of something abstracted from one body, and communicated to, or shared with another. The process is like kindling a great fire by a simple spark: here is no communication, but only *occasion* given to the exertion of powers inherent in the combustible matter. It appears probable, that the case is the same in magnetism; and that all that is performed in making a magnet is the excitement of powers already in the steel, or the giving occasion for their exertions; as burning the thread which ties together the two ends of a bow, allows it to unbend. This notion did not escape the sagacity of Dr. Gilbert; and he is at much pains to shew, that the *coitio magnetica* is a quality inherent in all magnetical bodies, and only requires the proper circumstance for its exertion. He is not very fortunate in his attempts to explain *how* it is developed by the vicinity of a magnet, and how this faculty, or actual exertion of this power, becomes permanent in one body, while in another it requires the constant presence of the magnet,

It is to Mr. Æpinus, of the Imperial Academy of St. Petersburgh, that we are indebted for the first really philosophical attempt to explain all these mysteries. We mentioned, under ELECTRICITY, the circumstance which suggested the first hint of this theory to Æpinus, *viz.* the resemblance between the attractions and repulsions of the tourmaline and of a magnet. A material cause of the elec-

tric phenomena had long been thought familiar to the philosophers. They had attributed them to a fluid which they called an electric fluid, and which they conceived to be shared among bodies in different proportions, and to be transferable from one to another. Dr. Franklin's theory of the Leyden phial, which led him to think that the faculty of producing the electrical phenomena depended on the deficiency as well as the redundancy of this fluid, combined with the phenomena of induced electricity, suggested to Æpinus a very perspicuous method of stating the analogy of the tourmaline and the magnet; which he published in 1758 in a paper read to the academy.

Reflecting more deeply on these things, Mr. Æpinus came by degrees to perceive the perfect similarity between all the phenomena of electricity by position and those of magnetism; and this led him to account for them in the same manner. As the phenomena of the Leyden phial, explained in Franklin's manner, shews that a body may appear electrical all over, by having less than its natural quantity of the electric fluid, as well as by having more, it seemed to follow, that it may also be so in respect to different parts of the same body; and therefore a body may become electrified in opposite ways at its two extremities, merely by abstracting the fluid from one end, and condensing it in the other; and thus may be explained the phenomena of induced electricity, where nothing appears to have been communicated from one body to the other. If this be the case, the two ends of a body rendered electric by induction should exhibit the same distinctions of phenomena that are exhibited by bodies wholly redundant and wholly deficient. The redundant ends should repel each other; so should the deficient ends; and a redundant part should attract a deficient. All these results of the conjecture tally exactly with observation, and give a high degree of probability to the conjecture. The similarity of these phenomena to the attractions of the dissimilar poles of a magnet, and the repulsions of the similar poles, is so striking, that the same mode of explanation.

forces itself on the mind, and led Mr. Æpinus to think, that the faculty of producing the magnetical phenomena belonged to a magnetical fluid, residing in all bodies susceptible of magnetism; and that the exertion of this faculty requires nothing but the abstraction of the fluid from one end of the magnetic bar, and its constipation in the other. And this conjecture was confirmed by observing, that in the induction of magnetism on a piece of iron, the power of the magnet is not diminished.

All these circumstances led Mr. Æpinus to frame the following hypothesis:

1. There exists a substance in all magnetic bodies, which may be called the magnetic fluid; the particles of which repel each other with a force decreasing as the distance increases.

2. The particles of magnetic fluid attract, and are attracted by the particles of iron, with a force that varies according to the same law.

3. The particles of iron repel each other according to the same law.

4. The magnetic fluid moves, without any considerable obstruction, through the pores of iron and soft steel; but is more and more obstructed in its motion as the steel is tempered harder; and in hard tempered steel, and in the ores of iron, it is moved with the greatest difficulty.

In consequence of this supposed attraction for iron, the fluid may be contained in it in a certain determinate quantity. This quantity will be such, that the accumulated attraction of a particle for all the iron balances, or is equal to, the repulsion of all the fluid which the iron contains. The quantity of fluid competent to a particle of iron is supposed to be such, that the repulsion exerted between it and the fluid competent to another particle of iron is also equal to its attraction for that particle of iron: And therefore the attraction between the fluid in an iron bar A for the iron of another bar B, is just equal to its repulsion

for the fluid in B; it is also equal to the repulsion of the iron in A for the iron in B. This quantity of fluid residing in the iron may be callad its NATURAL QUANTITY.

In conseqence of the mobility through the pores of the iron, the magnetic fluid may be abstracted from one end of a bar, and condensed in the other, by the agency of a proper external force. But this is a violent state. The mutual repulsion of the particles of condensed fluid, and the attraction of the iron which it has quitted, tend to produce a more uniform distribution. If we reflect on the law of action, we shall clearly perceive, that somewhat of this tendency must obtain in every state of condensation and rarefaction, and that there can be a perfect equilibrium only when the fluid is diffused with perfect uniformity. This, therefore, may be called the NATURAL STATE of the iron.

If the resistance opposed by the iron to the motion of the magnetic fluid be like that of perfect fluids to the motion of solid bodies, arising entirely from the communication of motion, there is no tendency to uniform diffusion so weak as not to overcome such resistance, and finally to produce this uniform distribution. But (as is more probable) if the obstruction resembles that of a clammy fluid, or of a soft plastic body like clay, some of the accumulation, produced by the agency of an external force, may remain when the force is removed; the diffusion will cease whenever the equalising force is just in equilibrio with the obstruction.

All the preceding circumstances of the hypothesis are so perfectly analogous to the hypothesis of Mr. Æpinus for explaining the electrical phenomena, which is given in detail under ELECTRICITY, that it would be superfluous to enter into a minute discussion of their immediate results. We therefore beg the reader to peruse that part of the article Electricity where the elements of Æpinus's hypothesis are delivered, and the phenomena of induced electricity explained (*viz.* from § 11. to 60. inclusive), and to suppose the

discourse to relate to the *magnetical* fluid. Let N, S, *n*, *s*, be considered as the overcharged and undercharged parts of a magnetical body, or the poles of a magnet, and of iron rendered magnetical by induction. We shall confine our observations in this place to those circumstances in which the mechanical phenomena of magnetism are limited by the circumstance, that magnets always contain their natural quantity of fluid; so that their action on iron, and on each other, depends entirely on its unequable distribution; as is the case with induced electricity.

312. Let the magnet NAS (Plate IV. fig. 12.), having its north pole NA overcharged, be set near to the bar *n* B *s* of common iron, and let their axes form one straight line. Then (as in the case of electrics) the overcharged pole NA acts on the bar B only by means of the redundant fluid which it contains. For that portion of its fluid, which is just sufficient for saturating the iron, will repel the fluid in B just as much as the iron in NA attracts it; and therefore the fluid in B sustains no change from this portion of the fluid in NA. In like manner, the pole SA acts on B only in consequence of the iron in SA, which is not saturated or attended by its equivalent fluid.

If the fluid in B is immoveable, even the redundant fluid in NA, and the redundant iron in SA, will produce no *sensible* effect on it: For every particle of iron in B is accompanied by as much fluid as will balance, by its repulsions and attractions, the attractions and repulsions of the equidistant particle of iron. But as the magnetical fluid in B is supposed to be easily moveable, it will be repelled by the redundant fluid in AN toward the remote extremity *n*, till the resistance that it meets with, joined to its own tendency to uniform diffusion, just balances the repulsion of AN. This tendency to uniform diffusion obtains as soon as any fluid quits its place; as has been sufficiently explained under Electricity, § 16, 17.

But, at the same time, the redundant iron in AS attracts the fluid in B, and would abstract it from B *n*, and condense

it into B *s*. This attraction opposes the repulsion now mentioned. But, because AS is more remote from every point of B than AN is from the same point, the repulsions of the redundant fluid in AN will prevail; and, on the whole, fluid will be propelled toward *n*, and will be rarefied on the part B *s*. But as to what will be the law of distribution, both in the redundant and deficient parts of B, it is plain that nothing can be said with precision. This must depend on the distribution of the fluid in the magnet NAS. The more diffused that we suppose the redundant fluid and matter in the magnet, the farther removed will the centres of effort of its poles be from their extremities; the smaller will be the action of AN and AS, the smaller will be their difference of action; and therefore the smaller will be the condensation in B *n*, and the rarefaction in B *s*. Hence we learn, in the outset of this attempt to explanation, that the action of a magnet will be so much the greater as its poles are more concentrated. This is agreeable to observation, and gives some credit to the hypothesis. We can just see, in a very general manner, that the fluid will be rarer than its natural state in *s*, and denser in *n*; that the change of density is gradual, and that the density may be represented by the ordinates of some line *c b d*, (Plate IV. fig. 13.) while the natural density is represented by the ordinates to the line C *b* D, parallel to *s n*. There will be some point B of the iron bar, where the fluid will be of its natural density, and the ordinate B *b* will meet the line *c b d* in the point of its intersection with CD.

All this action is internal and imperceptible. Let us inquire what will be the *sensible* external action. There is a superiority of attraction towards the magnet: For since the magnetic action is supposed to diminish continually by an increase of distance, the curve, whose ordinates represent the forces, has its convexity toward the axis. Also, the force of the poles AN, AS are equal at equal distances: For, by the hypothesis, the attraction and repulsion of an

individual particle are equal at equal distances; and the condensation in AN is equal to the deficiency in AS, by the same hypothesis; because NAS still contains its natural quantity of fluid. Therefore the action of both poles may be expressed by the ordinates of the same curve, and they will differ only by reason of their distances. We may therefore express the actions by the four ordinates M m, P p, N n, Q q, of Plate III. fig. 2.; of which the property (deduced from the single circumstance of its being convex toward the axis) is, that M m + Q q, is greater that P p + N n. There is therefore a surplus of attraction. It is only this surplus that is perceived. The fluid, moveable in B, but retained by it so as not to be allowed to escape, is pressed towards its remote end n by the excess P p — Q q of the repulsion of the redundant fluid in AN, above the attraction of the redundant iron in AS. This excess on every particle of the fluid is transmitted, by the common laws of hydrostatics, to the stratum immediately incumbent on the extremity n, and B is thus pressed away from A. But every particle of the solid matter in B is attracted towards A by the excess M m — N n of the attraction of the redundant fluid in AN above the repulsion of the redundant iron in AS; and this excess is greater than the other; for $\overline{m + q}$ is greater than $\overline{p + n}$.

The piece of common iron n B s is therefore attracted, in *consequence* of the fluid in it having been propelled towards its remote extremity, and distributed in a manner somewhat resembling its distribution in NAS. Now, in this hypothesis, magnetism is held to depend entirely on the distribution of the fluid. B has therefore become a magnet, has magnetism induced on it, and, only in consequence of this induction, is attracted by A.

Had we supposed the deficient, or south pole of A, to have been nearest to B, the redundant matter in AN would have attracted the moveable fluid in B more than the remoter redundant fluid in AS repels it: and, on this account, the magnetic fluid would have been constipated

in B *s*, and rarefied in B *n*. It would, in this case also, have been distributed in a manner similar to its situation in the magnet. And B would therefore have been a momentary magnet, having its redundant pole fronting the deficient or dissimilar pole of A. It is plain, that there would be the same surplus of attraction in this as in the former instance, and B would (on the whole) be attracted in consequence, and only in consequence, of having had a properly disposed magnetism induced on it by juxtaposition. The sensible attraction, in this case, is a *consequence* of the distribution now described; because, since the fluid constipated in the end next to A cannot quit B, the tendency of this fluid toward A must press the solid matter of B in this direction (by hydrostatical laws) more than this solid matter is repelled in the opposite direction.

Thus it appears, that the hypothesis tallies precisely with the induction of magnetism. We do not call this an explanation of the phenomenon; for the fact is, that it is the hypothesis that is explained by the phenomenon: That is, if any person be told that induced magnetism is produced by the action of a fluid, in consequence of its situation being changed, he will find, that in order to agree with the attraction of dissimilar, and the repulsion of similar poles, he must accommodate the fluid to the phenomena, by giving it the properties assigned to it by Æpinus.

314. But the agreement with this simplest possible case of the most simple example of induced magnetism, is not enough to make us adopt the hypothesis as adequate to the explanation of all the magnetic phenomena. We must confront the hypothesis with a variety of observations, to see whether the coincidence will be without exception.

When the key CB, in Plate III. fig. 8. is brought below the constipated north pole N of the magnet SAN, its own moveable fluid is propelled from C towards B, and is disposed in CB nearly after the same manner as in SAN. Therefore the redundant fluid in the lower end of the key repels

the moveable fluid in the wire BD more than the redundant matter in the upper end C attracts it; and thus the fluid is rarefied in the upper end of the wire BD, and condensed in its lower end D. CB and BD therefore are two temporary magnets, having their dissimilar poles in contact, or nearest to each other. This is all that is required for their attraction. This effect is promoted by the action of N on the wire BD, also propelling the fluid toward D; and thus increasing the mutual attraction of CB and BD. In like manner, when the key CB is held above the magnet, the moveable fluid in it is more attracted by the redundant matter in SA than it is repelled by the more remote redundant fluid in AN. The same thing happens to the fluid in the wire BD. Therefore CB and BD must attract each other; and the key will carry the wire, although the magnet is below it, and also attracts it. This singularity proceeds from the almost perfect mobility of the fluid in the two pieces of common iron, which renders their poles extremely constipated; whereas the hardness required for the fixed magnetism of the magnet prevents this complete constipation and rarefaction. This can be strictly demonstrated in the case of slender rods of iron; but we can shew, and experience confirms it, that in other cases, depending on the shape and the temper of the pieces, the wire will not adhere to the key, but to the magnet.

In the various situations and positions of the key and wire represented in Plate III. fig. 7. the actions of some of the poles on the moveable fluid in the iron are oblique in regard to the length of the pieces; but, since the moveable matter is supposed to be a fluid, it will still be propelled along the pieces, notwithstanding their obliquity, in the same manner as gravity makes water occupy the lower end of a pipe lying obliquely. If indeed the magnetic fluid could escape from the iron without any obstruction by the propulsion of the magnet, it could produce no attraction, or sensible motion, any more than light does in a transparent body. What is

demonstrated of the electric fluid in ELECTRICITY, No. 133. is equally true here. Why the fluid does not escape when it is so perfectly moveable, is a question of another kind, and will be considered afterwards; at present, the *hypothesis* is, that it does not escape.

If the key and wire have the position Plate III. fig. 10. No. 1. the fluid is expelled from the parts in contact, and is condensed in the remote ends. So far from attracting each other, the key and wire must repel. They are temporary magnets, having their similar poles fronting each other. They must repel each other, if presented in a similar manner to the south pole of the magnet.

If they be presented as in No. 2. Plate III. fig. 10. where the actions of both poles of the magnet are equal, the state of the fluid in them will not be affected. The redundant pole of the magnet repels the moveable fluid in both the key and the wire toward the upper ends: but the deficient pole acts equally on it in the opposite direction. It therefore remains uniformly distributed through their substance; and therefore they can exhibit no appearance of magnetism.

But if the key and wire be presented to the *same* part of the magnet, but in another position, as shewn in Plate III. fig. 8. No. 3. the fluid of the key will be abstracted from C, and condensed in B, by the joint action of both poles of the magnet. The same thing will happen in the wire BD. Here, therefore, we have two magnets with their dissimilar poles touching. They will attract each other strongly; and if carried gradually toward the upper or lower end of the magnet, they will separate before the point B arrives abreast of N or S. For similar reasons, the pieces of iron presented to the middle of the magnet, as in Plate III. fig. 10. will have one side a weak north pole, and the other side a weak south pole; but this will not be conspicuous, unless the pieces be broad.

This experiment shews, in a very perspicuous manner, the competency of the hypothesis to the explanation of the

phenomena. When the fluid is not moved, magnetism is not induced, even on the most susceptible substance.

When a piece of iron A (Plate III. fig. 10.), nearly as large as the magnet can carry, hangs at either pole, a large piece of iron B, brought near to the pole on the other side, should cause it immediately to fall. If S be the deficient pole, it causes the fluid in A to ascend to the top, and A is attracted; but, for the same reason, it causes the fluid in B to accumulate in its lower end. This redundant fluid must evidently counteract the redundant matter in S, in the induction of the magnetic state on A. Being more remote from A than S is, it cannot wholly prevent the accumulation in the upper end of A; but it renders it so trifling, that the remaining attraction thence arising cannot support the weight of A. This is a very instructive experiment.

But if, on the contrary, we bring a large piece of iron C below the heavy key A, this piece C will have its fluid accumulated in its upper end, both by the action of A on it, and by the action of the magnet. The attraction of the magnet for A should therefore be augmented; and a magnet should carry a heavier lump of iron when a great lump is beyond it. And it is clear (we think), for similar reasons, that the magnetism of the magnet itself in Plate III. fig. 11. should be increased by bringing a great lump of iron near its opposite pole: for the magnet differs from common iron only in the *degree* of the mobility of its fluid.

When a compass needle is placed opposite to the redundant pole N of a magnet AN (Plate IV. fig. 14.), it arranges itself magnetically. If a piece of common iron be now presented laterally to the near point of the needle, the redundant matter in the adjoining parts of the needle and the iron should make them repel; but if presented to the remote end, the redundant matter in the iron should attract the redundant fluid in that end of the needle, and that end should turn toward the iron.

A parcel of slender iron wires, carried by the pole of a magnet, as in Plate IV. fig. 15. should avoid each other. If

N be the redundant pole, the fluid in each wire will be driven to the remote end, where it must repel the similarly situated fluid of its neighbour. The same external appearance must be exhibited by pieces of wire hanging at the deficient pole of the magnet.

The redundant pole of a magnet A (Plate IV. fig. 16.) being held vertically above the centre of two pieces of common iron, moveable round a slender pin, renders the middle of each deficient, and their extremities redundant; therefore they should repel each other, and spread out. The same effect should be produced by the under charged pole of A.

The redundant pole of a magnet A being applied to one branch of the piece of forked iron NCS (Plate IV. fig. 17.), should drive the fluid into its remote parts C, and then the branch NC should be able to induce the magnetic state on a bit of iron D. But if the deficient pole S of another magnet B be applied to the other branch, these two actions should counteract each other at C, and the iron should remain indifferent, and fall.—Yet the magnet B alone would equally cause C to carry the piece of iron.

It is surely unnecessary to demonstrate, that the consequence of this hypothesis must be, that when a magnet puts any piece of iron into the magnetic state, its own magnetism is improved. For the induced magnetism of the iron is always so disposed as to give the fluid in the magnet a greater constipation where already condensed, and to abstract more fluid from the parts already deficient. If magnetism be produced by such a fluid, a magnet must always improve by lying any how among pieces of iron.

But the case may be very different when magnets are kept in each other's neighbourhood. When the overcharged poles of two magnets are placed fronting each other, the redundant fluid in each repels that in the other more than it attracts the remoter redundant iron. The magnets must therefore repel each other. Moreover, in rendering them mag-

netical, the repulsion of redundant fluid, or the attraction of redundant matter of some other magnet had been employed; and when the magnet was removed, some of the constipated fluid overcame the obstruction to its uniform diffusion, and escaped into the deficient pole; what remains is withheld by the obstruction, and the restoring forces are just in equilibrio with this obstruction. If we now add to them the repulsion of redundant fluid, directed toward the deficient pole, some more of the constipated fluid must be driven that way, and the magnet must be weakened. Nay, it may be destroyed, and even reversed, if one of the magnets be very powerful, and have its own magnetism very fixed; that is, if its fluid be very redundant, and meet with very great obstruction to its motion. Hence it also should follow, that the repulsion observed between two magnets should be weaker at the same distance than their attraction, and should follow a different law. For, in the course of the experiments, the situation of the fluid in the magnets is continually changing, and approaching to a state of uniform diffusion.

315. Let us now examine into the sensible effect of this fluid on a magnet which cannot move from its place, but can turn on its centre like a compass needle. This scarcely requires any discussion. We should only be repeating, with regard to the redundant fluid and redundant matter, what we formerly said in regard of north pole and south pole; the little magnet must arrange itself nearly in the tangent of a magnetic curve. But it requires a more minute investigation to determine what the sensible phenomenon should be when the fluid of the little magnet is perfectly moveable.

Suppose therefore a particle C (Plate IV. fig. 18.) of magnetic fluid, at perfect liberty to move in every direction, and acted on by the redundant and deficient poles of a magnet NAS. The redundant iron in S attracts C in the direction and with the force CF, while the redundant fluid in N repels it in the direction and with the force CD. By their joint action it must be urged in the direction and with the force CE, the

diagonal of the parallelogram CDEF, which must be accurately a tangent to a magnetic curve. If this particle of fluid belong to the piece of iron *n* C *s*, which lies in that very direction, it will unquestionably be pushed towards the extremity *n*. The same must happen to other particles. Hence it appears that a piece of common iron in this situation and position must become a magnet, and must retain this position; only the mechanical energy of the lever may change the equilibrium of the magnetic forces a little; because when the piece of iron *n* C *s* has any sensible magnitude, the action on its different points will be a little unequal, and may compose diagonals which divide a little from the tangent.

Should the iron needle chance not to have the exact position, but not deviate very far from it, it is also clear that the fluid, not being able to escape, will press on the side toward which it is impelled; and thus will cause the needle to turn on its pivot, and finally arrange itself in magnetical and mechanical equilibrium, deviating so much the less from a tangent to a magnetic curve as the piece of iron is smaller. Any piece of common iron, held in the neighbourhood of a magnet, will become more overcharged at one end and undercharged at the other, in proportion as the position of its length comes nearer to the tangent of a magnetic curve. A slender wire held perpendicular to this position, that is, perpendicular to the curve, should not acquire any sensible magnetism, either attractive or directive.

316. We surely need not now employ many words to shew that a parcel of iron filings, strewed round a magnet, should arrange themselves in the primary magnetic curves, or that when strewed round two magnets they should form the secondary or composite curves.

317. Let us now inquire more particularly into the modifications of this accumulation of magnetic fluid which may result from the nature of the piece of iron, as it is put into the magnetic state. The propelling force of A acts against the mutual repulsion of the particles of fluid in B, and also

against the obstruction to its motion through the pores of B. The greater this obstruction, the smaller will be the accumulation which suffices, in conjunction with the obstruction and the attraction of the deserted iron, to balance the propulsive force of the redundant fluid in the overcharged pole of A. This circumstance therefore must limit the accumulation that can be produced in a given time. Therefore the magnetism produced on soft steel or iron should be greater than that produced in hard steel at the same distance. Hence the great advantage of soft poles, or of armour, or of capping, to a loadstone, or to a bundle of hard bars. The best form and dimensions of this armour is certainly determinable by mathematical principles, if we knew the law of magnetical action, and the disposition of the magnetism in our loadstone; but these are too imperfectly known in all cases for us to pretend to give any exact rules. We must decide experimentally by making the caps large at first, and reducing them till we find the loadstone carry less; then make them a small matter larger. The chief things to be attended to are the purity, the uniformity, and the softness of the iron, and the closest possible contact.

If the obstruction resemble that to motion through a clammy fluid, the final accumulation in hard steel may be nearly equal to that in iron, but will require much longer time. Also, because such obstruction to the motion of the fluid will nearly balance the propelling force in parts that are far removed from the magnet, the accumulation will begin thereabouts, while the bar beyond is not yet affected. A redundant pole will be formed in that place. This will operate on what is *immediately* beyond it, driving the fluid farther on, and occasioning another accumulation at a small distance. This may produce a similar effect in a still smaller degree farther on. Thus the steel bar will have the fluid alternately condensed and rarefied, and contain alternate north and south poles. This state of distribution will not be permanent; the fluid will be gradually changing its place; these poles

will gradually advance along the bar, the remoter poles becoming gradually more diffuse and faint; and it will not be till after a very long time that a regular magnetism with two poles will be produced. To state mathematically the procedure of this mechanism would require many pages. Yet it may be done in some simple cases, as Newton has stated the process of aerial undulation. But we cannot enter upon the task in this limited dissertation. What is said in the article ELECTRICITY (§ 217, 218.) on the distribution of the electric fluid in an imperfect insulator, will assist the reader to form a notion of the state of magnetism during its induction. That such alternations proceed from such mechanism, we have sufficient proof in the instances mentioned in the former part of this article. The wave, or curl, produced on the surface of a clammy fluid, is a phenomenon of the same kind, and owing to similar causes.

When the magnet which has produced all these changes is removed, it is evident that a part of this accumulation will be undone again. The repulsion of the condensed fluid, and the attraction of the deserted iron, will bring back some of the fluid. But it is very evident, that a part of the accumulation will remain, by reason of the obstruction to its motion in returning; and this remainder must be so much the greater as the obstruction to the change of situation is greater. In short, we cannot doubt but that the magnetism which remains will be greater in hard than in spring tempered steel.

318. Thus have we traced the hypothesis in a great variety of circumstances and situations, and pointed out what should be the external appearance in each. We did not, in each instance, mention the perfect coincidence of these consequences with what is really observed, but left it to the recollection of the reader. The coincidence is indeed so complete, that it seems hardly possible to refuse granting that nature operates in this or some very similar manner. We get some confidence in the conjecture, and may even pro-

ceed to explain complicated phenomena by this hypothetical theory. We might proceed to shew, that the effects of all the methods practised by the artists in making artificial magnets are easy consequences of the hypothesis; but this is hardly necessary. We shall just mention some facts in those processes which have puzzled the naturalists.

1. A strong magnet is known to communicate the greatest magnetism to a bar of hard steel; but Muschenbroek frequently found, that a weak magnet would communicate more to a soft than to a hard bar.

Explanation. When the magnet is strong enough to impregnate both as highly as they are capable of, the hard bar must be the strongest; but if it can saturate neither, the spring-tempered bar must be left the most magnetical.

2. A strong magnet has sometimes communicated no higher magnetism than a weaker one; both have been able to saturate the bar.

3. A weak magnet has often impaired a strong one by simply passing along it two or three times; but a piece of iron always improves a magnet by the same treatment.

Explanation. When the north pole of a weak but hard magnet is set on the north pole of a strong one, it must certainly repel part of the fluid towards the other end, and thus it must weaken the magnet. When it is carried forward, it cannot repel this back again, because it is not of itself supposed capable of making the magnet so strong. But the end of a piece of iron, always acquiring a magnetism opposite to that of the part which it touches, must increase the accumulation of fluid where it is already condensed, and must expel more from those parts which are already deficient.

4. All the parts of the process of the double touch, as practised by Messrs. Mitchell and Canton, are easily explained by this hypothesis. A particle of fluid *p* (Plate IV. fig. 19.) situated in the middle between the two magnets, is repelled in the direction *p e* by the redundant pole of the mag-

net AN, whose centre of effort is supposed to be at C. It is attracted with an equal force in the direction $p\,d$ toward the centre of effort of the deficient pole of AS. By these combined actions it is impelled in the direction $p\,f$. Now it is plain that, although by increasing the distance between N and S, the forces with which these poles act on p are diminished, yet the compound force $p\,f$ may increase by the diminution of the angle $d\,p\,c$. If the action is as $\frac{1}{x^2}$, $p\,f$ will be greatest when $\frac{\text{Cos. } d\,p\,f}{d\,p^2}$ is a maximum, or (nearly) when $\text{Sin.}^2\, d\,p\,f \times \text{Cos. } d\,p\,f$ is a maximum: but this depends on the place of the centre of effort. We can, however, gather from this observation, that the nearer we suppose the centres of effort of the poles N and S to the extremities of the magnets, the nearer must they be placed to each other. But we must also attend to another circumstance; that by bringing the poles nearer together, although we produce a greater action on the intervening fluid, this action is exerted on a smaller quantity of it, and therefore a less effect may be produced. This makes a wider position preferable; but we have too imperfect a knowledge of the circumstances to be able to determine this with accuracy. The unfavourable action on the fluid beyond the magnets must also be considered. Yet all this may be ascertained with precision in some very simple instances, and the determination might be of service, if we had not a better method, independent of all hypotheses or theory; namely, to place the magnets at the distance where they are *observed* to lift the heaviest bar of iron; then we are certain that their action is most favourable, all circumstances being combined.

We also see a sufficient reason for preferring the position of the magnets employed by Mr Antheaume (and before him by Mr Servington Savery), in his process for making artificial magnets. The form of the parallelogram $d\,p\,e\,f$ is then much more favourable, the diagonal $p\,f$ being much longer,

We also see, in general, that, by the method of double touch, a much greater accumulation of fluid may be produced than by any other known process.

And, lastly, since no appearances indicate any difference between natural and artificial magnetism, this hypothesis is equally applicable to the explanation of the phenomena of natural magnetism; such as the position of the horizontal, and of the dipping needle, and the impregnation of natural loadstones.

Having such a body of evidence for the aptitude of this hypothesis for the explanation of phenomena, it will surely be agreeable to meet with any circumstances which render the hypothesis itself more probable. These are not wanting; although it must be acknowledged that nothing has yet appeared, besides the phenomena of magnetism, to give us any indication of the existence of such a fluid; but there are many particulars in their appearance which greatly resemble the mechanical properties of a fluid.

319. Heating a rod of iron, and allowing it to cool in a position perpendicular to the magnetic direction, destroys its magnetism. Iron is expanded by heat. If the particles of the magnetic fluid are retained between those of the iron, notwithstanding the forces which tend to diffuse them uniformly, they may thus escape from between the ferrugineous particles which withheld them. For similar reasons, magnetism should be acquired by heating a bar and letting it cool in the magnetic direction. But, besides this evident mechanical *opportunity* of motion, the union of fire (or whatever name the neologists may choose to give to the cause of expansion and of heat) with the particles of iron may totally change the action of those particles on the particles of fluid in immediate contact with them; nay, it may even change the sensible law of action between magnet and magnet. Of this no one can doubt who understands the application of mathematical science to corpuscular attraction (see

the Treatise on CORPUSCULAR ACTION in vol. I. BOSCOVICH.) A change may be produced in the action between magnets without any remarkable change happening in the actions within the magnet, and it may be just the reverse. The union of fire with the magnetic fluid may increase the mutual repulsion of its parts, as it does in all ærial fluids or gases. This alone would produce a dissipation of some magnetism. It may increase the attraction (at insensible distances) between the fluid and the iron, as it does in numberless cases in chemistry.

320. It is well known that violently knocking or hammering a magnet weakens its force, and that hammering a piece of iron in the magnetic direction will give it some magnetism. By this treatment the parts of the iron are put into a tremulous motion, alternately approaching and receding from each other. In the instants of their recess, the pent-up particles of the fluid may make their escape. A quantity of small shot may be uniformly mixed with a quantity of wheat, and will remain so for ever, if nothing disturb the vessel; but continue to tap it smartly with a stick for a long time, and the grains of small shot will escape from their confinements, and will all go to the bottom. We may conceive the particles of magnetic fluid to be affected in the same way. The same effect is produced by grinding or filing magnets and loadstones. The latter are frequently made useless by grinding them into the proper shape. This should be avoided as much as possible, and it should always be done in moulds made of soft iron and very massive; but this will not always prevent the dissipation of *strong* magnetism. As a farther reason for assigning this cause for the dissipation in such cases, it must be observed (Muschenbroek takes notice of it), that a magnet or loadstone may be ground at its neutral point without much damage. But we had the following most distinct example of the process. A very fine artificial magnet was suspended by a thread, with its south pole down. A person was employed to knock it incessantly with a piece of pebble, in such a manner as to make it

ng very clearly, being extremely hard and elastic. Its
agnetism was examined from time to time with a very small
ompass needle. In three quarters of an hour, its magnetism
as not only destroyed, but the lower end shewed signs of
north pole. The same magnet was again touched, and
ade as strong as before, and was then wound about very
ght with wetted whipcord, leaving a small part bare in the
iddle. It was again knocked with the pebble, but could
o longer ring. At the end of three quarters of an hour its
agnetism was still vigorous, and was not near gone after
vo hours and a quarter. We discharged a Leyden jar
oated with gold leaf) in the same way. It stood on the
p of an axis; and while this was turned round, the edge
as rubbed with a very dry cork filled with rosin, and fast-
ed to the end of a glass rod. This made the jar sound
e the glass of a harmonica. One of them was split in this
eration.

A small bar of steel was heated red hot and tempered hard
tween two strong magnets lying in shallow boxes filled
th water, and was more strongly impregnated in this way
an in any other that we could think of for a bar of that
ape. It has not yet been ascertained in what temperature
is most susceptible of magnetism, but it was considerably
tter than to be just visible in a dark place. It is no ob-
tion to our way of conceiving magnetism, that the fluid is
moveable or inactive when the iron is red hot. Either
these, or both of them, may result from the union with
e cause of heat. Even a particular degree of expansion
ay so change the law of action as to make it *immoveable*;
the union with caloric may render it *inactive* at all sensi-
e distances. We cannot but think, that some very in-
uctive facts might be obtained by experiments made
iron in the moment of its production, and changes in
ious chemical processes. All magnetism is gone when
s united with sulphur and arsenic in the greatest number

of ores; and when it is in the state of an ochre, rus
æthiops, or solution in acids; and when united with a
tringent substances, such as galls. When, and in wha
state, does it become magnetic? And whence comes the flui
of Æpinus? It were worth while to try, whether magne
have any influence in the formation or crystallization
the martial salts; and what will be their effect on iron whe
precipitated from its solutions by another metal, &c. &c.

321. There remains one remarkable fact to be taken no
tice of, which, in one point of view, is a confirmation of th
hypothesis, but in another presents considerable difficulties

It is well known that no magnet has ever been seen whic
has but one pole; that is, on the hypothesis of Æpinus
which is wholly redundant, or wholly deficient. If all mag
netism be either the immediate or the remote effect of th
great magnet contained in the earth, and if it be produce
by induction, without any communication of substance, bu
only by changing the disposition of the fluid already in th
iron, we never should see a magnet with only one pole. I
must be owned, that we never can make such a magnet b
any of the processes hitherto described; but the existenc
of such does not seem impossible. Supposing a magnet, o
the most regular magnetism, having only two poles; an
that we cut it through at the neutral point, or that we cu
or break off any part of it—the fact is (for the experimen
has been tried ever since men began to speculate about mag
netism), that each part becomes an ordinary magnet, wit
two poles, one of which is of the same kind as before the se
paration. The question now is, What should happen ac
cording to the theory maintained by Æpinus?—*Tentam
Theor. Elect. et Magnetismi*, p. 104, &c.

Let NAS (Plate IV. fig. 20.) be a magnet, of which N i
the overcharged pole. Let the ordinates of the curve DAI
express the difference between the natural density of th
fluid, in a state of uniform diffusion, and its density as it i

really disposed in the magnet*. The area $p\ n$ ND will there express the quantity of redundant fluid in the part n N, and the area q ES m expresses the fluid wanting in the part S m. The intersection A marks that part of the magnet where the fluid is of its natural density. Suppose the part N n to be separated from the rest, containing the redundant fluid ND $p\ n$. The tendency of this fluid to escape from the iron with which it is connected will be greater (Mr. Æpinus thinks) than before; because its tendency to quit the magnet formerly was repressed by the attractions of the redundant matter contained in AS. This is certainly true of the extremity N; nay, perhaps of all the old external surface. Fluid will therefore escape. Suppose that so much has quitted the iron that the point n has the fluid of its natural density, as is represented in No. 3. there is still a force operating at n, tending to escape, arising from the repulsion of all the redundant fluid n DN. If this be sufficient for overcoming the obstruction, it will really escape, and the iron will be left in the state represented by No. 4. with an overcharged part f N, and an undercharged part $f\ n$.

In like manner, the tendency of the magnetic fluid surrounding the magnet to enter into its deficient pole, will be greater when it is separated from the other, not being checked by the repulsion of the redundant fluid in that other.

* Coulomb found that the magnetic action diminished from the north or south pole of a magnet towards the neutral point according to the following law.

Distance from the North or south Pole in Inches.	*Intensity of action.*
0	165+
1	90
2	48
3	23
4.5	9
6	6

The magnet with which these results were obtained, was cylindrical, and was 27 inches long, and two lines in diameter. See Biot's *Traité de Physique*, vol. iii. p. 73, 74, 79.—Ed.

Mr. Æpinus relates some experiments which he made on this subject. The general result of them was, that the moment the parts were separated, each had two poles, and that the neutral point of each magnet was much nearer to the place of their former union than to the other ends. In a quarter of an hour afterwards, the neutral points had advanced nearer to their middle, and continued to do so, by very small steps, for some hours, and sometimes days, and finally were stationary in their middles.

322. We acknowledge, that this reasoning does not altogether satisfy us, and that the gradual progress of the neutral point toward the middle of each piece, although agreeable to what should result from an escape of fluid, is not a proof of it. We know already, that the induction of magnetism is a progressive thing; and we should have expected this change of the situation of the neutral point, whatever be the nature of magnetism. There is something similar to this, and perhaps equally puzzling, in the immediate recovery of magnetism which has been weakened by heat; it is partly recovered on cooling.

But our chief difficulty is this: At the point A (Plate IV. fig. 20.) every thing is in equilibrium before the fracture. The particle A is repelled by the redundant fluid in AN, and attracted by the redundant matter in AS; yet it does not move, for the magnetism is supposed to have permanency. Therefore the obstruction at A cannot be overcome by the united repulsion of AN and attraction of AS. Nor can the obstruction at N be overcome by the difference of these two forces. Now suppose AS annihilated. The change made on the state of things at A is surely greater than that at N, because the force abstracted is greater, the distance being less. It does not clearly appear, therefore, that the removal of AS should occasion an efflux at N. This, however, is not impossible; because the fluid may be so disposed, by great constipation near N, and no great excess of density near A, that a smaller change at N may pro-

duce an efflux there. But surely the tendency to escape at A must now be diminished, instead of being greater after the fracture. And if any escape from N, this will still more diminish that tendency to escape from A. It does not therefore appear a clear consequence of the general theory, that the constipated fluid should escape; and more particularly, that A should become deficient. And with respect to the entry of fluid into the other fragment, and its becoming overcharged at *m*, the reasoning seems still less convincing. The steps of the physical process in the two parts of the original magnet are by no means convertible or counterparts of each other. There is nothing in the part AS to resemble the force of repulsion really exerting itself in the corresponding point of AN. There would be, if there were a particle of fluid in that place; but there is not. The tendency therefore of external fluid to enter there, does not resemble the tendency of the internal fluid to expand and dissipate. It is true, indeed, the discourse should be confined to points of the surface. But the internal motion must also be considered; and the great objection always remains, namely, that the obstruction at A (No. 1.) or at *n* (No. 3.) is sufficient to prevent the passage of a particle of fluid from the pole AN into the pole AS, when urged by the repulsion of the fluid in the one, and the attraction of the iron in the other; and yet will not prevent the escape of a particle when one of those causes of motion is removed. Add to this, that the whole hypothesis assumes as a principle, that the resistance to escape from any point is greater than the obstruction to motion through the pores. This is readily granted; for however great we suppose the attraction, in the limits of physical contact, it will be no obstruction to motion through the pores, because the particle is equally affected by the opposite sides of the pores; whereas, in quitting the body altogether, there is nothing beyond the body to counteract the attraction by which it is retained.

There seems something wanting to accommodate this beautiful hypothesis of Mr. Æpinus to this remarkable phenomenon; and the coincidence is otherwise so complete, that we are almost obliged to conclude that it is merely a deficiency, arising from our not having a sufficient knowledge of the law of magnetic action. This is quite sufficient: For it may be strictly demonstrated, that if the magnetic action decreases in a higher ratio than that of the squares of the distances, the permanency of the fluid in any particular disposition has scarcely any dependence on the particles at any sensible distance, and is affected only by the *variations* of its density (See ELECTRICITY, § 217. for a case somewhat similar.) Therefore, if the fluid be so disposed, that its density may be represented by the ordinates of such a curve as is drawn in Plate IV. fig. 20, having its two extremities concave toward the axis, and a point of contrary flexure at A, the tendency to escape at A will be the greatest possible; and when the magnet is broken at A (No. 1.) or when the fluid has taken the arrangement represented by No. 3, it *cannot* stop there, and *must* become deficient in that part. Now, it must be acknowledged, that we are not absolutely certain that the magnetic action is in the precise inverse duplicate ratio of the distance*. All that we are certain of is, that it is much nearer to it than to either the inverse simple or inverse duplicate ratio. We own ourselves rather disposed to ascribe the present difficulty to our ignorance of some circumstances, purely mathematical, overlooked, or mistaken, than to think a conjecture unfounded, which tallies so accurately with such a variety of phenomena.

We may here observe, that we are not altogether satisfied with Æpinus' form of the experiment. He did not break a magnet; he set two steel bars end to end, and touched them

* The experiments of Coulomb prove incontestibly that this is the law of magnetic action.—ED.

as one bar, making the magnetism perfectly regular; he then separated them, and found that each had two poles. But was he certain that, when joined, they made but one magnet? We have sometimes succeeded in doing this, as we thought, by the curves of iron filings; but on putting the needle with which we were examining their polarity into proper situations, we sometimes found it in the second intersection of the secondary curves, shewing that the bars were really two magnets, and not one.

On the other hand, when a piece is broken off from a a magnet, the succussion and elastic tremor into which the parts are thrown, and even the bending previous to the fracture, may give opportunity to a dissipation, which could not otherwise happen. The parts should be separated by corrosion in an acid, and the gradual change of magnetism should be carefully noted. The writer of this article has made some experiments of this nature, the results of which present some curious observations: but they are not yet brought to a conclusion that is fit to be laid before the public*.

* The very remarkable fact, that every fragment of a magnet, whether it is taken from its north or its south extremity, is itself a magnet, with two distinct and opposite poles, appears incapable of being explained by the theory of Æpinus.

The hypothesis of Coulomb however accounts for it in a very simple manner. This eminent philosopher considers every particle of iron as a small magnet, possessing a north and a south pole of equal intensity, and hence a magnet is an assemblage of such infinitely small magnets, having their axes parallel to the axis of the great magnet which they compose, in the same manner as a tourmaline is composed of a number of elementary crystals disposed symmetrically, and exhibiting the two opposite electricities. When therefore a fragment is broken from the north pole of a magnet, the extremity of the fragment nearest the neutral point, becomes a south pole, because the southern polarity of the particles at this extremity is no longer counteracted by the northern polarity of the other particles with which they were formerly in contact.

In the *Philosophical Transactions* for 1816, we have given a full account of a series of very remarkable phenomena in crystallized glass, which are precisely the same as those which take place in magnets. A rectangular plate of glass, whether it is transiently or permanently crystallized, exhibits in its action upon

323. Mr. Prevôst of Geneva, in a dissertation on the origin of magnetic forces, endeavours to give a theory which obviates the only difficulty in that of Æpinus; but it is incomparably more complex, employing two fluids, which by their union compose a third, which he calls combined fluid. There is much ingenuity, and even mathematical address, in adjusting the relative properties of those fluids. But some of them are palpably incompatible; *ex. gr.* the particles of each attract each other, but those of the other kind most strongly; yet they are both elastic like air. This is surely inconceivable.—Granting this, however, he suits his different attractions, so that a strong elective attraction of the combined fluid for iron decomposes part of the fluid in the iron, and each of its ingredients occupies opposite ends of the bar: then will the bars approach or recede, according as the near ends contain a different or the same ingredient. All this is operated without repulsion.

But the whole of this is mere accommodation, like Æpinus's, but so much more complex, that it requires very intense contemplation to follow the author through the consequences. Add to this, that his attractions are operated by another fluid, infinitely more subtle than either of those already mentioned, every particle of these being, as it were, a world, in comparison of those of the other. In short, he adopts all the extravagant suppositions of Le Sage of Geneva, and every thing is ultimately impulsion. Nor is the contrivance for obviating the difficulty (so often mentioned) at all clear and convincing; and it is equally gratuitous with

light, a north pole at each edge, and a south pole in the middle; a neutral point lying between the south pole and each north one. If a rectangular slip is cut with a diamond from this plate, so that the whole slip before its separation may possess only northern polarity, yet no sooner is it separated than it exhibits the same properties as the original plate, having a north pole at each edge, and a south pole in the middle. For farther information on the subject of this note, see Hauy's *Natural Philosophy*, vol. ii. p. 90. *Phil. Trans.* 1816, pp. 82, 98, 99, and the Edinburgh Encyclopedia, Art. Heat, vol. x. p. 677.—Ed.

the rest. We cannot think this hypothesis at all intitled to the name of *explanation*

324. This must serve for an account of the hypothesis of Æpinus. The philosophical reader will see, that however exactly it may tally with every phenomenon, it cannot be called an explanation of the phenomena; because it is the phenomena which explain the hypothesis, or give us the characters of the magnetic fluid, if such fluid exists. But we are not obliged to admit this existence, as we admit that to be the true decyphering of a letter which makes sense of it. In that case we know both parts of the subject—the characters and the sounds; but are ignorant which corresponds to which. Did we see a fluid abstracted from one part of a bar and constipated in another, and perceive the abstraction and constipation always accompanied by the observed attractions and repulsions, the rules of philosophical discussion, nay, the constitution of our own mind, would oblige us to assign the one as the cause or occasion of the other. But this important circumstance is wanting in the present case. We think, however, that it merits a close attention; and we entertain great hopes of its being one day completed, by including this single exception.

At the same time, it must be owned, that it gives no extension of knowledge; for it can have no greater extension than the phenomena on which it is founded, and cannot, without risk of error, be applied to an untried case, of a kind dissimilar in its nature to the phenomena on which it is founded. We doubt not but that its ingenious author would have said, that a bit broken off from the north pole of a magnet would be wholly a north pole, if he had not known that the fact was otherwise.

But this hypothesis greatly aids the imagination in conceiving the process of the magnetical phenomena. The more we study them, the more do they appear to resemble the protrusion of a fluid through the parts of an obstructing body. It proceeds gradually. It may be, as it were, over-

done, and regorges when the propelling cause is removed. The motion is aided by what we know to aid other obstructed motions. As a fluid would be constipated in all protuberances, so the faculty of producing the phenomena is greater in all such situations, &c. &c. This, joined to the impossibility of speaking, with clearness of conception, of the propagation of powers without the protrusion of something in which they inhere, gives it a hold of the imagination which is not easily shaken off.

To say that nothing is explained when the attraction of the fluid is not explained, and that this is the main question, gives us little concern. We offer no explanation of this attraction, more than of the attraction of gravity. There is nothing contrary to the laws of human intellect, nothing inconsistent with the rules of reasoning, in saying, that things are so constituted, that when two particles are together, they separate, although we are ignorant of the immediate cause of their separation. Those who think that all motion is performed by impulsion, and who explain magnetism by a stream of fluid circulating round the magnet, must have another fluid to impel this fluid into its curvilineal path; for they insist, that the planets are so impelled. Then they must have a third fluid to deflect the vertical motions of the second, and so on without end. This is evident, and it is absurd.

325. We conclude with desiring the reader to remark, that the explanation which we have given of the magnetical phenomena is independent of the hypothesis of Æpinus, or any hypothesis whatever. We have narrated a variety of very distinguishable facts, and have marked their distinctions. We have been able to reduce them to general classes; and even to groupe those classes into others still more general; and at last, to point out one which is discoverable in them all. This is giving a philosophical theory, in the strictest sense of the word; because we shew, in every case, the modification of the general fact which allots it this or that particular place in that classification. Thus we have

shewn that the polarity or directive power of magnets is only a modification of the general fact of attraction and repulsion. Dr. Gilbert's theory of *terrestrial* magnetism is indeed a hypothesis, and we enounced it as such. It only claims probability, and we apprehend that a very high degree of credit will be given to it.

We hope that many of our readers will have their curiosity excited by the account we have given of Æpinus's theory. To such we earnestly recommend the serious perusal of his book *Tentamen Theoriæ Electricitatis et Magnetismi, Auct. F. Æpino*, Petropoli, 1759. Van Swinden has included a very good abstract of it in his 2d volume *Sur l'Electricité*, written by Professor Steiglehner of Ratisbon or Ingoldstadt. The mathematical part is greatly simplified, and the whole is presented in a very clear and accurate manner. Mr. Van Swinden is a professed foe to all hypotheses; but he is not moderate, and we wish that we could say that he is candid. He attacks every thing; and takes the opportunity of every analogy pointed out by Æpinus between magnetism and electricity to repeat the first sentence of his dissertation, namely, that magnetism and electricity are not the same; a thing that Æpinus also maintains. But he even charges Æpinus with a mistake in his fundamental equations, which invalidates his whole theory. He says that Æpinus has omitted one of the acting forces assumed in his hypothesis. This is a most groundless charge; and we own that we cannot conceive how Van Swinden could fall into such a mistake. The Abbe Hauy of the French Academy has also published an abridgment of Æpinus's theory, with many excellent remarks, tending to clear the theory of the only defect that has been found in it. This work was much approved of, and recommended by the Academy. We have not had the good fortune to see a copy of it *.

* This excellent work, which we would recommend to those who are not much versed in mathematical knowledge, is entitled *Exposition raisonnée de la Theorie de l'Electricité et du Magnetisme, d'apres les principes de M. Æpinus*, Paris, 1787. Ed.

326. The reader cannot but have remarked the close analogy between the magnetical phenomena and those of induced electricity; indeed, all the phenomena of attraction and repulsion are the same in both. The mechanical composition of those actions produces a directive power and a polarity, in electrical as well as in magnetical bodies. We can make an electrical needle which will arrange itself, with respect to the overcharged and undercharged ends of a body electrified by mere position, just as a compass needle is arranged by a magnet. We can touch a stick of stealing wax in the manner of the double touch, so as to give it poles of considerable force and durability. As a red hot steel bar acquires permanent poles by quenching it near a magnet, so melted wax accquires them by freezing in the neighbourhood of a positive and negative electric. Some have inferred a sameness of origin of these two species of powers from those various circumstances of resemblance; but the original causes seem to be distinct on many accounts. Electricity is common to all bodies. The cause of magnetism can operate only on iron. Although lightning or an electrical shock gives polarity to a needle, we need not infer the identity of the cause, because the polarity which it gives is always the same with that given by great heat; and there is always intense heat in this operation. The phenomenon which looks the most like an indication of identity of the origin of electricity and magnetism is the direction of the rays of the aurora borealis—they converge to the same point of the heavens to which the elevated pole of the dipping needle directs itself. But this is by no means a sufficient foundation for establishing a sameness. Electricity and magnetism may, however, be related by means of some powers hitherto unknown. But we are decidedly of opinion, that the electric and magnetic fluid are totally different, although their mechanical actions are so like that there is hardly a phenomenon in the one which has not an exact counterpart in the other. But we see them both operating, with all their

marks of distinction, in the same body; for iron and loadstones may be electrified, like any other body, and their magnetism suffers no change or modification. We can set these two forces in opposition or composition, just as we can oppose or compound gravity with either. While the iron filings are arranging themselves round a magnet, the mechanical action of electricity may be employed either to promote or hinder the arrangement. They are therefore distinct powers, inherent in different subjects.

327. But there are abundance of other phenomena which shew this diversity. There is nothing in magnetism like a body overcharged or undercharged *in toto*. There is nothing which indicates the presence of the fluid to the other senses—nothing like the spark, the snap, the visible dissipation; because the magnetic fluid enters into no union with air, or any thing but iron. There is nothing resembling that inconceivably rapid motion which we see in electricity; the quickest motion of magnetism seems inferior (even beyond comparison) to the slowest motion along any electric conductor. Therefore there is no possibility of discharging a magnet as we discharge a coated plate. Indeed, the resemblance between a magnet and a coated plate of glass is exceedingly slight. The only resemblance is between the magnet and an inconceivably thin stratum of the glass, which stratum is positive in one side and negative in the other. The only perfect resemblance is between the induced magnetism of common iron, and the induced electricity of a conductor.

The following seem the most instructive dissertations on magnetism, either as valuable collections of observations, or as judicious reasonings from them, or as the speculations of eminent or ingenious men concerning the nature of magnetism.

Gilbertus de Magnete, Lond. 1600, fol.

Æpini Tentamen Theoriæ Magn. et Electr.

Eberhard's Tentam. Theor. Magnetismi 1720.

Dissertations sur l'aimant, par du Fay, 1728.

Muschenbroek Dissert. Physico Experimentalis de Mag
nete.

Pieces qui ont emporté le prix de l'Acad. des Sciences
Paris sur la meilleure construction des Boussoles de declina
tion. Recueil des pieces couronnées, tom. v.

Euleri opuscula, tom. iii. continens Theoriam Magnetis,
Berlin, 1751.

Æpini Oratio Academica, 1758.

Æpini item Comment. Petrop. nov. tom. x.

Anton. Brugmanni tentam. Phil. de materia Magnetica
Francqueræ, 1765.

There is a German translation of this work by Eisenbach
with many very valuable additions.

Scarella de Magnete, 2 tom. fol.

Van Swinden Tentamina Magnetica, 4to.

Van Swinden sur l'Analogie entre les phenomenes Elec
triques et Magnetiques, 3 tom. 8vo.

Dissertation sur les Aimans artificielles par Antheaume.

Experiences sur les Aimans artificielles par Nicholas, Fus
1782.

Essai sur l'Origine des Forces Magnetiques par Mr. Pre
vost.

Sur les Aimans artificielles par Rivoir, Paris, 1752.

Dissertatio de magnetismo par Sam. Klingenstier et Jo
Brander, Holm. 1752.

Description des Courants Magnetiques, Strasbourg, 175

Traité de l'Aiman par Dalancé, Amst 1687.

Besides these original works, we have several dissertа
tions on magnetical vortices by Des Cartes, Bernoulli, Eule
Du Tour, &c. published in the collections of the works o
those authors, and many dissertations in the memoirs of di
ferent academies; and there are many popular treatises b
the traders in experimental philosophy in London and Pari
Dr. Gowin Knight, the person in Europe who was mos
eminently skilled in the knowledge of the phenomena, als

published a dissertation intitled, *An attempt to explain the Phenomena of Nature by two principles, Attraction and Repulsion*, Lond. 1748, 4to, in which he has included a theory of magnetism. It is a very curious work, and should be studied by all those who have recourse without scruple to the agency of invisible fluids, when they are tired of patient thinking. They would there see what thought and combination are necessary before an invisible fluid can be really fitted for performing any office we choose to assign it. And they will get real instruction as to what services we may expect of such agents, and from what tasks they must be excluded. The Doctor's theory of magnetism is very unlike the rest of the performance; for he does not avail himself of the vast apparatus of propositions which he had established, and adopts without any nice adjustment the most common notions of an impulsive vortex. Both the production and maintenance of this vortex, and its mode of operation, are irreconcileable with the acknowledged laws of impulsion*.

* Iron, steel, nickel, and cobalt, have generally been regarded as the only metals which are magnetic. M. Coulomb, however, in the month of May, 1802, announced to the National Institute of France, that when small needles about 7 or 8 millimetres long, and about half a millimetre thick, and made of *any substance whatever*, were suspended between the two opposite poles of two strong magnets, they always arranged themselves in the line joining the poles. The needles which were tried, were made of gold, silver, lead, copper, tin, glass, wood, chalk, bone, and other organic and inorganic substances. The only way of explaining this remarkable fact, is to suppose either that all substances in nature are magnetic, or that they owe this property to the presence of a quantity of iron or other magnetic metal, too small to be ascertained by chemical tests. Some of these experiments were repeated by Dr. Thomas Young at the Royal Institution. The memoir of Coulomb, which contains an account of these discoveries, has never been published; but M. Biot, who had access to it, has given a full account of the leading results in his *Traité de Physique*, tom. iii. chap. ix. p. 117. The English reader will find the subject fully treated in the article Magnetism in the Edinburgh Encyclopædia.—Ed.

APPENDIX.

INVESTIGATION OF THE MAGNETIC CURVES.

I HAVE been favoured with the following investigatio of the curves, to which a needle of indefinite minutene will be a tangent, by Mr. Playfair, Professor of Mathem tics in the University of Edinburgh.

Two magnetical poles being given in position, the for of each of which is supposed to be as the mth power of th distance from it reciprocally, it is required to find a curv in any point of which a needle (indefinitely short) bein placed, its direction, when at rest, may be a tangent to th curve?

1. Let A and B (Plate IV. fig. 21.) be the poles of a ma net, C any point in the curve required; then we may su pose the one of these poles to act on the needle only by r pulsion, and the other only by attraction, and the directio of the needle, when at rest, will be the diagonal of a para lelogram, the sides of which represent these forces. Ther fore, having joined AC and BC, let AD be drawn parall to BC, and make $\frac{1}{AC^m} : \frac{1}{BC^m} :: AC : AD$; join CD, th CDF will touch the curve in C.

2. Hence an expression for AF may be obtained. Fo by the construction, $AD = \frac{AC^{m+1}}{BC^m}$, and since $BC : AD$ $BF : FA$, and $BC - AD : AD :: AB : AF$, we have A $= \frac{AB \times AC^{m+1}}{BC^{m+1} - AC^{m+1}}$.

3. A fluxionary expression for AF may also be found in terms of the angles CAB, ABC. In CF take the indefinitely small part CH, draw AH, BH, and from C draw CL perpendicular to AH and CK to BH. Draw also BG and AM at right angles to FH. Let the angles CAB $= \varphi$, and CBA $= \psi$; then CAH $= \varphi$, and CBH $= -\dot{\psi}$; also CL $= AC \times \dot{\varphi}$, and CK $= -BC \times \dot{\psi}$. Now HC : CL : : AC : AM $= \frac{AC^2 \times \dot{\varphi}}{HC}$; and for the same reason BG $= -\frac{BC^2 \times \dot{\psi}}{HC}$. Therefore since AF : FB : : AM : BG, AF : FB : : $\frac{AC^2 \times \dot{\varphi}}{HC} : -\frac{BC^2 \times \dot{\psi}}{HC}$, and AF : AB : : sin. $\dot{\psi}^2 \dot{\varphi}$: $-$ sin. $\psi^2 \dot{\varphi}$ $-$ sin. $\varphi^2 \dot{\psi}$; wherefore if AB $= a$, AF $= \frac{-a \varphi \text{ sin. } \psi^2}{\dot{\psi} \text{ sin. } \varphi^2 + \dot{\varphi} \text{ sin. } \psi^2}$.

4. If this value of AF be put equal to that already found, a fluxionary equation will be obtained, by the integration of which the curve may be constructed. Because AF $= \frac{AB \times AC^{m+1}}{BC^{m+1} - AC^{m+1}}$; and since AC $= \frac{a \text{ sin. } \psi}{\text{sin. } (\dot{\varphi} + \dot{\psi})}$, and BC $= \frac{a \text{ sin. } \varphi}{\text{sin. } (\varphi + \dot{\psi})}$, we have by substitution AF $= \frac{a \text{ sin. } \psi^{m+1}}{\text{sin. } \varphi^{m+1} - \text{sin. } \psi^{m+1}} = -\frac{a \dot{\varphi} \text{ sin. } \psi^2}{\dot{\psi} \text{ sin. } \dot{\varphi}^2 + \varphi \text{ sin. } \psi^2}$. Hence, sin. $\varphi^2 \times \dot{\psi}$ sin. $\psi^{m+1} + \dot{\varphi}$ sin. $\psi^{m+3} = -$ sin. $\psi^2 \times \dot{\varphi}$ sin. $\varphi^{m+1} + \varphi$ sin. ψ^{m+3}, and therefore $\dot{\psi}$ sin. $\psi^{m-1} = -\dot{\varphi}$ sin. φ^{m-1}; and also, $\int \dot{\psi}$ sin. $\psi^{m-1} + \int \dot{\varphi}$ sin. $\varphi^{m-1} = C$.

5. These fluents are easily found when m is any whole positive number.

If $m = 1$, we have $\dot{\psi} + \dot{\varphi} = o$.
$m = 2$, $\dot{\psi}$ sin. $\psi + \varphi$ sin. $\varphi = o$
$m = 3$, $\dot{\psi}$ sin. $\psi^2 + \dot{\varphi}$ sin. $\varphi^2 = o$.
$m = 4$, $\dot{\psi}$ sin. $\psi^3 + \dot{\varphi}$ sin. $\varphi^3 = o$, &c.

Therefore, &c.

Also if $m = 1$, $\varphi + \psi = C$.

$m = 2$, cos. φ + cos. $\psi = C$.

$m = 3$, —sin. $2\varphi + 2\varphi$ —sin. $2\psi + 2\psi = C$.

$m = 4$, cos. $3\varphi - 9$ cos. φ + cos. 3. $\psi - 9$. cos. $\Psi = C$, &c. &c.

The first of the above equations belongs to a segment of a circle described upon AB, which therefore would be the curve required if the magnetical force were inversely as the distances.

If the magnetical force be inversely as the square of the distance, that is, if $m = 2$, cos. φ + cos. ψ is equal to a constant quantity. Hence if, beside the points A and B any other point be given in the curve, the whole may be described. For instance, let the point E (Plate IV. fig. 22.) be given in the curve, and in the line DE which bisects AB at right angles. Describe from the centre A a circle through E, viz. QER; then AD being the cosine of DAE to the radius AE, the sum of the cosines of $\varphi \times \psi$ will be everywhere (to the same radius) = 2 AD = AB. Therefore to find E′, the point in which any other line AN, making a given angle with AB, meets the curve, draw from N, the point in which it meets the circumference of the circle QER, NO, perpendicular to AB, so that AO may be the cosine of NAO, and from O toward A take OP = AB, then AP will be the cosine of the angle ABE′; so to find BE′ draw PQ perpendicular to AP, meeting the circle in Q; join AQ, and draw BE′ parallel to AQ, meeting AE′ in E′, the point E′ is in the curve. In this way the other points of the curve may be found.

The curve will pass through B, and will cut AB at an angle of which the cosine = RB. If then E be such, that AE = AB, the curve will cut AB at right angles. If E″ be more remote from A, the curve will make with AB an obtuse angle toward D; in other cases it will make with it an acute angle.

A construction somewhat more expeditious may be had by describing the semicircle AFB, cutting AE in F, and AE′ in N, and describing a circle round A, with the distance AL = 2 AF, cutting AE′ in *b*. If BG be applied in the semicircle AFB = N *b*, BG must cut AN in a point E′ of the curve, because AN + BG = 2 AF, and AN and GB are cosines of the angles at A and B.

As the lines AN and BG may be applied either above or below AB, there is another situation of their intersection E′. Thus A *n* being applied above, and B *g* below, the intersection is in *e*′. The curve has a branch extending below A; and if D *e* be made = DE, and B *e* be drawn, it will be an assymptote to this branch There is a similar branch below B. But these portions of the curve evidently suppose an opposite direction of one of the two magnetic forces, and therefore have no connection with the position of the needle.

VARIATION OF THE COMPASS*.

THE *variation of the Compass*, is the deviation of the magnetic or mariner's needle from the meridian or true north and south line. On the continent it is called the *declination* of the magnetic needle; and this is a better term for reasons which will afterwards appear.

Our readers know, that the needle of a mariner's compass is a small magnet, exactly poised on its middle, and turning freely in a horizontal direction on a sharp point, so that it always arranges itself in the plane of the magnetic action.

About the time that the polarity of the magnet was first observed in Europe, whether originally, or as imported from China, the magnetic direction, both in Europe and in China, was nearly in the plane of the meridian. It was therefore an inestimable present to the mariner, giving him a sure direction in his course through the pathless ocean. But by the time that the European navigators had engaged in their adventurous voyages to far distant shores, the deviation of the

* It is necessary to remind the reader, that the following article on the VARIATION OF THE COMPASS, was published a considerable time before the article on MAGNETISM.—ED.

compass needle from the meridian was very sensible even in Europe; and it is somewhat surprising that the Dutch and Portuguese navigators did not observe it on their own coasts. The son of Columbus positively says, that it was observed by his father in his first voyage to America, and made his companions so anxious lest they should not find the way back again to their own country, that they mutinied and refused to proceed. It is surprising that any should doubt of its being known to this celebrated navigator, because he even endeavours to account for it by supposing the needle to be always directed to a fixed point of the heavens, different from the pole of the world, which he calls the *point attractive*. It is at any rate certain that Gonzales Oviedo and Sebastian Cabot observed it in their voyages. Indeed it could not possibly escape them; for in some parts of their several tracks the needle deviated above 25 degrees from the meridian; and the rudest dead reckoning, made on the supposition of the needle pointing due north and south, must have thrown the navigators into the utmost confusion. It would indeed be very difficult for them, unprepared for this source of error, to make any tolerable guess at its quantity, till they got to some place on shore, where they could draw a meridian line. But we know that spherical trigonometry was at that time abundantly familiar to the mathematicians of Europe, and that no person pretended to take the command of a ship bound to a distant port that was not much more informed in this science than most masters of ships are now-a-days. It could not be long, therefore, before the methods were given them for discovering the variation of the compass by observation of Amplitudes and Azimuths, as is practised at present. But the deviation of the compass from the meridian was not generally allowed by mathematicians, who had not yet become sensible of the necessity of quitting the Aristotelian trammels, and investigating nature by experiments. They rather chose to charge the navigators with inaccuracy in their observations than the schoolmen with error in princi-

ples. Pedro de Medina at Valladolid, in his *Arte de Navigar*, published in 1545, positively denies the variation of the compass. But the concurring reports of the commanders of ships on distant voyages, in a few years, obliged the landsmen in their closets to give up the point; and Martin Cortez, in a treatise of navigation, printed at Seville, before 1556, treats it as a thing completely established, and gives rules and instruments for discovering its quantity. About the year 1580 Norman published his discovery of the *dip* of the needle, and speaks largely of the horizontal deviation from the plane of the meridian, and attributes it to the attraction of a point, not in the heavens, but in the earth, and describes methods by which he hoped to find the place. To the third, and all the subsequent editions of Norman's book *(called the new attractive)*, was subjoined a dissertation by Mr. Burroughs, comptroller of the navy, on the variation of the compass, in which are recorded the quantity of this deviation in many places; and he laments the obstacle which it causes to navigation by its total uncertainty previous to observation. The author indeed offers a sort of rule for computing it *a priori*, founded on some conjecture as to its cause; but, with the modesty and candour of a gentleman, acknowledges that this is but a guess, and intreats all navigators to be assiduous in their observations, and liberal in communicating them to the public; conjuring them to consider, that an interested regard to their own private advantage, by concealing their knowledge, may prove the shipwreck of thousands of brave men. Accordingly observations were liberally contributed from time to time, and were published in the subsequent treatises on navigation.

But in 1635 the mariners were thrown into a new and great perplexity, by the publication of a *Discourse mathematical on the variation of the Magnetical Needle*, by Mr. Henry Gillebrand, Gresham professor of astronomy. He had compared the variations observed at London by Burroughs,

Gunter, and himself, and found that the north end of the mariner's needle was gradually drawing more to the westward. For Norman and Burroughs had observed it to point about $11\frac{1}{2}$ degrees to the east of the north in 1580; Gunter found its deviation only $6\frac{1}{4}$ in 1622, and he himself had observed only 4° in 1634; and it has been found to deviate more and more to the westward ever since, as may be seen from the following little table in Waddington's Navigation.

Variation at London.

1576	Norman	11°15′ East
1580	Burroughs	11 17
1622	Gunter	6 12
1634	Gillebrand	4 5
1662		0 0
1666	Sellers	0 34 West
1670		2 06
1672		2 30
1700		9 40
1720		13 —
1740		16 10
1760		19 30
1774		22 20
*1778	Phil. Trans.	22 11
1804	June, Phil. Trans.	24 8.4
1806	June, Phil. Trans.	24 8.6
1807	Sept. Phil. Trans.	24 10.2
1808	Phil. Trans.	24 10
1811	Sept. Phil. Trans.	24 12 2″
1812	October, Phil. Trans.	24 16 30
1813	June, Col. Beaufoy	24 22 17
1814	June, Ditto	24 22 48
1815	June, Ditto	24 27 18
1815	June, Phil. Trans.	24 18

* The measures of the variation after 1778, have been added principally from the *Philosophical Transactions*.—Ed.

Mr. Bond, teacher of mathematics in London, and employed to take care of and improve the impressions of the popular treatises of navigation, about the 1650, declared, in a work called the "Seaman's Kalendar," that he had discovered the true progrees of the deviation of the compass; and published in another work, called the "Longitude Found," a table of the variation for 50 years. This was however a very gratuitous sort of prognostication, not founded on any well-grounded principles; and though it tallied very well with the observations made in London, which showed a gradual motion to the westward at the rate of —.12 annually, by no means agreed with the observations made in other places. See Phil. Trans. 1668.

But this glad news to navigators soon lost its credit: for the inconsistency with observation appeared more and more every day, and all were anxious to discover some general rule, by which a near guess at least might be made as to the direction of the needle in the most frequented seas. Mr. Halley, one of the first geometers and most zealous philosophers of the last century, recommended the matter in the most earnest manner to the attention of Government; and, after much unwearied solicitation, obtained a ship to be sent on a voyage of discovery for this very purpose. He got the command of the ship, in which he repeatedly traversed the Atlantic Ocean, and went as far as the 50th degree of southern latitude. See his very curious speculations on this subject in the Phil. Trans. 1683 and 1692.

After he had collected a prodigious number of observations made by others, and compared them with his own, he published in 1700 a synoptical account of them in a very ingenious form of a sea chart, where the ocean was crossed by a number of lines passing through those places where the compass had the same deviation. Thus, in every point of one line there was no variation in 1700; in every point of another line the compass had 20 degrees of east variation; and in every point of a third line it had 20º of west varia-

tion. These lines have since been called *Halleyan lines*, or curves. This chart was received with universal applause, and was undoubtedly one of the most valuable presents that science has made to the arts. But though recommended with all the earnestness which its importance merited, it was offered with the candour and the caution that characterises a real philosopher ardently zealous for the propagation of true knowledge. Its illustrious author reminds the public of the inaccuracy of observations collected from every quarter, many of them made by persons not sufficiently instructed, nor provided with proper instruments; many also without dates, and most of them differing in their dates, so that some reduction was necessary for all, in order to bring them to a common epoch; and this must be made without having an unquestionable principle on which to proceed. He said, that he plainly saw that the change of variation was very different in different places, and in the same place at different times; and confesses that he had not discovered any general principle by which these changes could be connected.

Halley's *Variation Chart*, however, was of immense use; but it became gradually less valuable, and in 1745 was exceedingly erroneous. This made Messrs. Mountain and Dodson, fellows of the Royal Society, apply to the Admiralty and to the great trading companies for permission to inspect their records, and to extract from them the observations of the variations made by their officers. They got all the assistance they could demand; and, after having compared above 50,000 observations, they composed new variation charts, fitted for 1745 and 1756.

The polarity of the magnetic needle, and a general though intricate connection between its positions in all parts of the world, naturally causes the philosopher to speculate about its cause. We see that Cortez ascribed it to the attraction of an eccentric point, and that Bond thought that this point was placed not in the heavens, but in the earth. This notion made the basis of the famous Theory of Magnetism of

Dr. Gilbert of Colchester, the first specimen of experimental philosophy which has been given to the public. It was published about the year 1600: he was an intimate acquaintance of the great experimental philosopher lord Bacon, and proceeded entirely according to the plan laid down by that illustrious leader in his *Novum Organum Scientiarum.*

Gilbert asserted that the earth was a great magnet, and that all the phenomena of the mariner's compass were the effects of this magnetism. He shewed at least that these phenomena were precisely such as would result from such a constitution of the earth; that is, that the positions of the mariner's needle in different parts of the earth were precisely the same with those of a small magnet similarly situated with respect to a very large one. Although he had made more magnetic experiments than all that had gone before him put together, still the magnetical phenomena were but scantily known till long after. But Gilbert's theory (for so it must be truly esteemed) of the magnetical phenomena is now completely confirmed. The whole of it may be understood from the following general proposition.

Let NS (Plate IV. fig. 23.) be a magnet, of which N is the north and S the south pole: Let *n s* be any oblong piece of iron, poised on a point **c** like a compass needle. It will arrange itself in a position *n c s* precisely the same with that which would be assumed by a compass needle of the same size and shape, having *n* for its north and *s* its south pole. And while the piece of iron remains in this position, it will be in all respects a magnet similar to the real compass needle. The pole *n* will attract the south pole of a small magnetised needle, and repel its north pole. If a paper be held over *n s*, and fine iron-filings be strewed on it, they will arrange themselves into curves issuing from one of its ends and terminating at the other, in the same manner as they will do when strewed on a paper held over a real compass needle. But this magnetism is quite temporary; for if the piece of iron *n s* be turned the other way, placing *n* where *s* now is,

it will remain there, and will exhibit the same phenomena. We may here add, that if $n\,s$ be almost infinitely small in comparison of NS, the line $n\,s$ will be in such a position that if $s\,a$, $s\,b$, be drawn parallel to N c, S c, we shall have $s\,a$ to $s\,b$ as the force of the pole N to the force of the pole S. And this is the true cause of that curious disposition of iron filings when strewed round a magnet. Each fragment becomes a momentary magnet, and arranges itself in the true magnetic direction; and when so arranged, attracts the two adjoining fragments, and co-operates with the forces which also arrange them.

Now, to apply this theory to the point in hand.—Let $n\,s$ (Plate IV. fig. 24.) be a small compass needle, of which n is the north and s the south pole: let this needle be poised horizontally on the pin $c\,d$; and let $n\ s'$ be the position of the *dipping needle*. Take any long bar of common iron, and hold it upright, or nearly so, as represented by AB. The lower end B will repel the pole n and will attract the pole s, thus exhibiting the properties of a north pole of the bar AB. Keeping B in its place, turn the bar round B′ as a centre, till it come into the position A′B′ nearly parallel to $n'\,s'$. You will observe the compass needle $n\,s$ attract the end B′ with either pole n or s, when B′A′ is in the position B′$\propto$ perpendicular to the direction $n'\,s'$ of the dipping needle: and when the bar has come into the position B′A′, the upper end B′ will shew itself to be a south pole by attracting n and repelling s. This beautiful experiment was exhibited to the royal society in 1673 by Mr. Hindshaw.

From this it appears, that the great magnet in the earth induces a momentary magnetism on soft iron precisely as a common magnet would do. Therefore (says Dr. Gilbert) it induces permanent magnetism on magnetisable ores of iron, such as loadstones, in the same manner as a great loadstone would do; and it affects the magnetism already imparted to a piece of tempered steel precisely as any other great magnet would.

Therefore the needle of the mariner's compass in every part of the world arranges itself in the magnetic direction, so that, if poised as a dipping needle should be, it will be a tangent to one of the curves N c S of Plate IV. fig. 23. The horizontal needle being so poised as to be capable of playing only in a horizontal plane, will only arrange itself *in the plane* of the triangle N c S. That end of it which has the same magnetism with the south pole S of the great magnet included in the earth will be turned towards its north pole N. Therefore what we call the north pole of a needle or magnet really has the magnetism of the south pole of the great primitive magnet. If the line NS be called the axis, and N and S the poles of this great magnet, the plane of any one of these curves N c S will cut the earth's surface in the circumference of a circle, great or small, according as the plane does or does not pass through the centre of the earth.

Dr. Halley's first thought was, that the north pole of the great magnet or loadstone which was included in the bowels of the earth was not far from Baffin's Bay, and its south pole in the Indian ocean south-west from New Zealand. But he could not find any positions of these two poles which would give the needle that particular position which it was observed to assume in different parts of the world; and he concluded that the great terrestrial loadstone had four irregular poles (a thing not unfrequent in natural loadstones, and easily producible at pleasure), two of which are stronger and two weaker. When the compass is at a great distance from the two north poles, it is affected so as to be directed nearly in a plane passing through the strongest. But if we approach it much more to the weakest, the greater vicinity will compensate for the smaller absolute force of the weak pole, and occasion considerable irregularities. The appearances are favourable to this opinion. If this be the real constitution of the great magnet, it is almost a desperate task to ascertain by computation what will be the position of the needle. Halley seems to have despaired; for he was both

an elegant and a most expert mathematician, and it would have cost him little trouble to ascertain the places of two poles only, and the direction which these would have given to the needle. But to say what would be its position when acted on by four poles, it was necessary to know the law by which the magnetic action varied by a variation of distance; and even when this is known, the computation would have been exceedingly difficult.

In order to account for the change of variation, Dr. Halley supposes this internal magnet not to adhere to the external shell which we inhabit, but to form a nucleus or kernel detached from it on all sides, and to be so poised as to revolve freely round an axis, of which he hoped to discover the position by observation of the compass. The philosopher will find nothing in this ingenious hypothesis inconsistent with our knowledge of nature. Dr. Halley imagined that the nucleus revolved from east to west round the same axis with the earth. Thus the poles of the magnet would change their positions relatively to the earth's surface, and this would change the direction of the compass needle.

The great Euler, whose delight it was always to engage in the most difficult mathematical researches and computations, undertook to ascertain the position of the needle in every part of the earth. His dissertation on this subject is to be seen in the 13th volume of the Memoirs of the royal Academy of Berlin, and is exceedingly beautiful, abounding in those analytical *tours d'addresse* in which he surpassed all the world. He has reduced the computation to a wonderful simplicity.

He found, however, that four poles would engage him in an analysis which would be excessively intricate, and has contented himself with computing for two only; observing that this supposition agrees so well with observation, that it is highly probable that this is the real constitution of the terrestrial magnet, and that the coincidence would have been perfect if he had hit on the due positions of the two poles.

He places one of them in lat. 76° north, and long. 96° west from Teneriffe. The south pole is placed in lat. 58° south, and long. 158° west from Teneriffe. These are their situations for 1757.—Mr. Euler has annexed to his dissertation a chart of Halleyan curves suited to these assumptions, and fitted to the year 1757.

It must be acknowledged, that the *general* course of the variations according to this theory greatly resembles the real state of things; and we cannot but own ourselves highly indebted to this great mathematician for having made so fine a first attempt. He has improved it very considerably in another dissertation in the 22d volume of these memoirs. But there are still such great differences, that the theory is of no service to the navigator, and it only serves as an excellent model for a farther prosecution of the subject. Since that time another large variation chart has been published, fitted to a late period; but the public has not sufficient information of the authorities or observations on which it is founded.

The great object in all these charts is to facilitate the discovery of a ship's longitude at sea. For the lines of variation being drawn on the chart, and the variation and the latitude being observed at sea, we have only to look on the chart for the intersection of the parallel of observed latitude and the Halleyan curve of observed variation. This intersection must be the place of the ship. This being the purpose, the Halleyan lines are of great service; but they do not give us a ready conception of the direction of the needle. We have always to *imagine* a line drawn through the point, cutting the meridian in the angle corresponding to the Halleyan line. We should learn the general magnetic affections of the globe much better if a number of magnetic meridians were drawn. These are the intersections of the earth's surface with planes passing through the magnetical axis, cutting one another in angles of 5° or 10°. This would both shew us the places of the magnetic poles much more clearly, and would, in every place, show us at once the di-

rection of the needle. In all those places where these magnetical curves touch the meridians, there is no variation; and the variation in every other place is the angle contained between these magnetical meridians and the true ones.

The program of a work of this kind has been published by a Mr. Churchman, who appears to have engaged in the investigation with great zeal and considerable opportunities. He had been employed in some operations connected with surveys of the back settlements in North America. It is pretty certain that the north magnetic pole (or point, as Mr. Churchman chooses to call it) is not far removed from the stations given it by Halley and Euler; and there seems no doubt but that in the countries between Hudson's Bay and the western coasts of North America the needle will have every position with respect to the terrestrial meridian, so that the north end of a compass needle will even point due south in several places. Mr. Churchman has solicited assistance from all quarters, to enable him to traverse the whole of that inhospitable country with the compass in his hand. It was greatly to be wished that our gracious sovereign, who has always shewn such a love for the promotion of nautical science, and who has so munificently contributed to it, already enriching the world with the most valuable discoveries, and thus laying posterity under unspeakable obligations; it were greatly to be wished that he would put this almost finishing stroke to the noble work, and enable Mr. Churchman, or some fitter person, if such can be found, to prosecute this most interesting inquiry. Almost every thing that can be desired would be obtained by a few *well-chosen* observations made in those regions. It would be of immense advantage to have the *dips* ascertained with great precision. These would enable us to judge at what depth under the surface the pole is situated; for the well informed mechanician, who will study seriously what we have said about the magnetical curves, will see that a compass needle, when compared with the great terrestrial magnet, is but as a particle

of iron-filings compared to a very large artificial magnet. Therefore, from the position of the dipping needle, we may infer the place of the pole, if the law of magnetic action be given; and this law may be found by means of other experiments which we could point out.

Mr. Churchman has adopted the opinion of only two poles. According to him, the north pole lies (in 1800) in Lat. 58° N. and Long. 134° west from Greenwich, very near Cape Fair-weather; and the south pole lies in Lat. 58° S. and Lon. 165° E. from Greenwich*. He also imagines that the north pole has moved to the eastward, on a parallel of latitude, about 65° since the beginning of last century (from 1600), and concludes that it makes a revolution in 1096 years. The southern pole has moved less, and completes its revolution in 2289 years. This motion he ascribes to some influences which he calls *magnetic tides*, and which he seems to consider as celestial. This he infers from the changes of variation. He announces a physical theory on this subject, which, he says, enables him to compute the variation with precision for any time past or to come; and he even gives the process of trigonometrical computation illustrated by examples. But as this publication (entitled *The Magnetic Atlas*, published for the Author, by Darton and Harvey, 1794) is only a program, he expresses himself obscurely, and somewhat enigmatically, respecting his theory, waiting for encouragement to make the observations which are necessary for completing it. He has, in the mean time, accompanied his account of the theory with a chart, in the form of gussets, for covering a globe of 15 inches diameter,

* M. Biot has shewn, from a comparison of the observations of La Peyrouse, Humboldt, Bayly, and Lacaille, that the magnetic equator is a great circle of the terrestrial sphere, inclined 12° to the equator, and has its western node in 113° 14′ of west longitude, near the island Gallego, and the other node in 293° 14′. By examining however the observations of Bayly and Cook, made in 1777, he has found that the magnetic equator is irregular, crossing the equator at least three, and perhaps four times. The magnetic poles are in 78° of lat. and 203° 14′ of west long.—Ed.

objecting very justly to the great distortion which Wright's charts occasion in every part near the poles. This distortion is such as totally to change the appearance of the curves in those very places where their appearance and magnitude are of the greatest moment.

Mr. Churchman has also accompanied his work with the returns which he has received from several persons eminent for their rank or learning, to whom he had applied for encouragement and assistance. They are polite, but, we think, not so encouraging as such zeal in such a cause had good reason to expect. We acknowledge that there are circumstances which justify caution in promises of this nature. His proffers are very great, and not qualified with any doubt. Some of his proofs are not very convincing, and there are some considerable defects in the scientific part. He speaks in such terms of the magnetic influences as plainly lead us to conclude that they resemble, in effect at least, the ordinary actions of magnets. He speaks of the influence of one pole being greater than that of the other: and says, that in this case the magnetic equator, where the needle will be parallel to the axis, will not be in the middle between the poles. This is true of a common magnet. He must therefore abide by this supposition in its other consequences. The magnetic meridians must be planes passing through this axis, and therefore must be circles on the surface of the earth. This is incompatible with the observations; nay, his charts are so in many places, particularly in the Pacific Ocean, where the variations by his chart are three times greater than what has been oberved.—His parallels of dip are still more different from observation, and are incompatible with any phenomena that could be produced by a magnet having but two poles. His rules of computation are exceedingly exceptionable. He has in fact but one example, and that so particular, that the mode of computation will not apply to any other. This circumstance is not taken notice of in the enunciation of his first problem; and the reader

is made to imagine that he has got a rule for computing the variation, whereas all the rules of calculation are only running in a circle. The variation computed for the port of St. Peter and Paul in Kamtschatka, by the rule, is ten times greater than the truth. This is like the artifice of a book-maker. We do not meet with addition to our knowledge on the subject. The author seems to know something of Euler's merit; but instead of prosecuting the subject in his way, he gives us an uninteresting account of the surmises of a number of obscure writers about the difficulty of the task; and we think that Mr. Churchman has left us as much in the dark as ever. The observation of the connection of the polarity of the needle with the aurora borealis occurred to the writer of this article as early as 1759, when a midshipman on board the Royal William in the River St. Laurence. Some of the gentlemen of the quarter-deck are still alive, and may remember this circumstance being pointed out to them one evening, when at anchor off the Isle aux Coudres, during a very brilliant aurora borealis. The point of the heavens to which all the rays of light converged was precisely that which was opposite to the south end of the dipping-needle. The observation was inserted in the St. James's Chronicle, and afterwards (about 1776) in the London Chronicle, with a request to navigators to take notice of it, and communicate their observations.

For our own part, we have little hopes of this problem ever being subjected to accurate calculation. We believe, indeed, that there is a cosmical change going on in the earth, which will produce a progressive change in the variation of the needle; and we see none more likely than Dr. Halley's notion. There is nothing repugnant to our knowledge of the universe in the supposition of a magnetic nucleus revolving within this earth; and it is very easy to conceive a very simple motion of revolution, which shall produce the very motion of the sensible poles which Mr. Churchman contends for. We need only suppose that the magnetical

axis of this nucleus is not its axis of revolution. It may not even bisect that axis; and this circumstance will cause the two poles to have different degrees of motion in relation to the shell which surrounds it.

But this regular progress of the magnet within the earth may produce very irregular motions of the compass needle, by the intervention of a third body susceptible of magnetism. The theory of which we have just given a hint comes here to our assistance. Suppose NS (Plate IV. fig. 25.) to represent the primitive magnet in the earth, and *n s* to be a stratum of iron-ore susceptible of magnetism. Also let *n′ s′* be another small mass of a similar ore; and let their situations and magnitudes be such as is exhibited in the figure. The fact will be, that *n* will be the north pole and *s* the south pole of the great stratum, and *n′* and *s* will be the north and south poles of the small mass or loadstone. Any person may remove all doubts as to this, by making the experiment with a magnet NS, a piece of iron or soft tempered steel *n s*, and another piece *n s*. The well informed and attentive reader will easily see, that by such interventions every conceivable anomaly may be produced. While the great magnet makes a revolution in any direction, the needle will change its position gradually, and with a certain regularity; but it will depend entirely on the size, shape, and situation, of these intervening masses of magnetisable iron-ore, whether the change of variation of the compass shall be such as the primitive magnet alone would have produced, or whether it shall be of a kind wholly different.

Now, that such intervening disturbances *may* exist, is past contradiction. We know that even on the film of earth which we inhabit, and with which only we are acquainted, there are extensive strata or otherwise disposed masses of iron-ores in a state susceptible of magnetism; and experiments made on bars of hard tempered steel, and on bits of such ores, assure us that the magnetism is not induced on such bodies in a moment, but propagated gradually along the mass.—That such disturbances do actually exist, we

have many relations. There are many instances on record of very extensive magnetic rocks, which affect the needle to very considerable distances. The island of Elba in the Mediterranean is a very remarkable instance of this. The island of Cannay also, on the west of Scotland, has rocks which affect the needle at a great distance.

A similar effect is observed near the Feroe islands in the North Sea; the compass has no determined direction when brought on shore. *Journ. des Sçavans*, 1679, p. 174.

In Hudson's Straits, in latidude 63°, the needle has hardly any polarity. *Ellis' Voyage to Hudson's Bay.*

Bouguer observed the same thing in Peru. Nay, we believe that almost all rocks, especially of whin or trappe stone, contain iron in a proper state.

All this refers only to the thin crust through which the human eye has occasionally penetrated. Of what may be below we are ignorant; but when we see appearances which tally so remarkably with what would be the effects of great masses of magnetical bodies, modifying the general and regularly progressive action of a primitive magnet, whose existence and motion is inconsistent with nothing that we know of this globe, this manner of accounting for the observed change of variation has all the probability that we can desire. Nay, we apprehend that very considerable changes may be produced in the direction of the compass needle even without the supposition of any internal motion. If the great magnet resembles many loadstones we are acquainted with, having more than two poles, we know that these poles will act on each other, and gradually change each other's force, and consequently the direction of the compass. This process, to be sure, tends to a state of things which will change no more. —But the period of human history, or of the history of the race of Adam, may make but a small part of the history of this globe; and therefore this objection is of little force.

There can be no doubt of the operation of the general terrestrial magnetism on every thing susceptible of magnetic properties; and we cannot hesitate to explain in this way

many changes of magnetic direction which have been observed. Thus, in Italy, Father de la Torré observed, that during a great eruption of Vesuvius the variation was 16° in the morning, at noon it was 14°, and in the evening it was 10°, and that it continued in that state till the lava grew so dark as no longer to be visible in the night ; after which it slowly increased to $13°_{\frac{2}{3}}$, where it remained. Daniel Bernoulli found the needle change its position 45′ by an earthquake. Professor Muller at Manheim observed that the declination of the needle in that place was greatly affected by the earthquake in Calabria. Such streams of lava as flowed from Hekla in the last dreadful eruption must have made a transference of magnetic matter that would considerably affect the needle. But no observations seem to have been made on the occasion; for we know that common iron-stone, which has no effect on the needle, will, by mere cementation with any inflammable substance, become magnetic. In this way Dr. Knight sometimes made artificial loadstones.—But these are partial things, and not connected with the general change of variation now under consideration.

We have said so much on this subject, chiefly with the view of cautioning our readers against too sanguine expectations from any pretensions to the solution of this great problem. We may certainly gather from these observations, that even although the theory of the variation should be completed, we must expect (by what we already know of magnetism in general) that the disturbances of the needle, by local causes intervening between it and the great influence by which it is chiefly directed, may be so considerable as to affect the position of the compass needle in a very sensible manner: for we know that the metallic substances in the bowels of the earth are in a state of continual change, and this to an extent altogether unknown.

There is another irregularity of the mariner's needle that we have taken no notice of, namely, the daily variation. This was first observed by Mr. George Graham in 1722

(*Philosophical Transactions*, No. 383.) and reported to the Royal Society of London. It usually moves (at least in Europe) to the westward from 8 morning till 2 P. M. and then gradually returns to its former situation. The diurnal variations are seldom less than 0° 5′, and often much greater. Mr. Graham mentions (*Philosophical Transactions*, No. 428.) some observations by a Captain Hume, in a voyage to America, where he found the variation greatest in the afternoon. This being a general phenomenon, has also attracted the attention of philosophers. The most detailed accounts of it to be met with are those of Mr. Canton, in *Philosophical Transactions*, Vol. LI. Part 1. p. 399, and those of Van Swinden, in his *Treatise on Electricity and Magnetism*.

It appears from Canton's observations, that although there be great irregularities in this diurnal change of position of the mariner's needle, there is a certain average, which is kept up with considerable steadiness. The following table shews the average of greatest daily change of position in the different months of the year, observed in Mr. Canton's house, Spittal Square, in 1759.

January	7′ 8″	July	13′14″
February	8 85	August	12 19
March	11 27	Sept.	11 43
April	12 26	October	10 36
May	13 —	Nov.	9
June	13 21	Dec.	6 58*

Mr. Canton attempts to account for these changes of position, by observing that the force of a magnet is weakened

* The following results deduced from very accurate observations made by Colonel Beaufoy at Hackney, in west long. 6′ $\frac{52}{100}$ in time, and north lat. 51°, 32′, 40″, will shew the greatest daily variation in the year 1813.

1813.	March 30.	15′ 0″		Sept. 29.	9′ 45″
	April 18.	20 57		Oct. 10.	11 49
	May 3.	16 35		Nov. 21.	5 48
	June 29.	19 55		Dec. 3.	5 53
	July 19.	12 55	1814.	Jan. 11.	6 9
	Aug. 27.	9 37		Feb. 10	11 47

The two variations, of which the preceding numbers are the differences, were taken about eight o'clock in the morning, and two in the afternoon. The reader will find a very full series of magnetic observations by Colonel Beaufoy, in Dr. Thomson's *Annals of Philosophy*, vols. i. ii. iii. &c.—Ed.

by heat. A small magnet being placed near a compass needle, ENE from it, so as to make it deflect 45° from the natural position, the magnet was covered with a brass vessel, into which hot water was poured. The needle gradually receded from the magnet $\frac{1}{4}$ths of a degree, and returned gradually to its place as the water cooled. This is confirmed by uniform experience.

The parts of the earth to the eastward are first heated in the morning, and therefore the force of the earth is weakened, and the needle is made to move to the westward. But as the sun warms the western side of the earth in the afternoon, the motion of the needle must take the contrary direction.

But this way of explaining by a change in the force of the earth supposes that the changing cause is acting in opposition to some other force. We do not know of any such. The force, whatever it is, seems simply to produce its own effect, in deranging the needle from the direction of terrestrial magnetism. If Æpinus's theory of magnetic action be admitted, *viz.* that a bar of steel has magnetism induced on it by propelling the quiescent and mutually repelling particles of magnetic fluid to one end, or attracting them to the other, we may suppose that the sun acts on the earth as a magnet acts on a piece of soft iron, and in the morning propels the fluid in the north-west parts. The needle directs itself to this constipated fluid, and therefore it points to the eastward of the magnetic north in the afternoon. And (to abide by the same theory) this induced magnetism will be somewhat greater when the earth is warmer; and therefore the diurnal variation will be greatest in summer. This change of position of the constipated fluid must be supposed to bear a very small ratio to the whole fluid, which is naturally supposed to be constipated in one pole of the great magnet in order to give it magnetism. Thus we shall have the diurnal variation a very small quantity. This is departing, however, from the principle of Mr. Canton's explanation; and indeed we cannot see how the weakening the general force

of the terrestrial magnet should make any change in the needle in respect to its direction: nor does it appear probable that the change of temperature produced by the sun will penetrate deep enough to produce any sensible effect on the magnetism. And if this be the cause, we think that the derangements of the needle should vary as the thermometer varies, which is not true. The other method of explaining is much better, if Æpinus's theory of magnetic attraction and repulsion be just; and we may suppose that it is only the secondary magnetism (i. e. that of the magnetisable minerals) that is sensibly affected by the heat; this will account very well for the greater mobility of the fluid in summer than in winter.

A great objection to either of these explanations is the prodigious diversity of the diurnal variations in different places. This is so very great, that we can hardly ascribe the diurnal variation to any change in the magnetism of the primitive terrestrial magnet, and must rather look for its cause in local circumstances. This conclusion becomes more probable, when we learn that the deviation from the meridian and the deviation from the horizontal line are not affected at the same time. Van Swinden ascribes them solely to changes produced on the needles themselves. If their magnetism be greatly deranged by the sun's position, it may throw the magnetic centre away from the centre of the needle's motion, and thus may produce a very small change of position. But if this be the cause, we should expect differences in different needles. Van Swinden says, that there are such, and that they are very great; but as he has not specified them, we cannot draw any conclusion.

But, besides this regular diurnal variation, there is another, which is subjected to no rule. The aurora borealis is observed (in Europe) to disturb the needle exceedingly, sometimes drawing it several degrees from its position. It is always observed to increase its deviation from the meridian, that is, an aurora borealis makes the needle point more westerly. This disturbance sometimes amounts to six or

seven degrees, and is generally observed to be greatest when the aurora borealis is most remarkable.

This is a very curious phenomenon, and we have not been able to find any connection between this meteor and the position of a magnetic needle. It is to be observed, that a needle of copper or wood, or any substance besides iron, is not affected. We long thought it an electric phenomenon, and that the needle was affected as any other body balanced in the same manner would be; but a copper needle would then be affected. Indeed it may still be doubted whether the aurora borealis be an electric phenomenon. They are very frequent and remarkable in Sweden; and yet Bergman says, that he never observed any electric symptoms about them, though in the mean time the magnetic needle was greatly affected.

We see the needle frequently disturbed both from its general annual position, and from the change made on it by the diurnal variation. This is probably the effect of auroræ boreales which are invisible, either on account of thick weather or day-light. Van Swinden says, he seldom or never failed to observe auroræ boreales immediately after any anomalous motion of the needle; and concluded that there had been one at the time, though he could not see it Since no needle but a magnetic one is affected by the aurora borealis, we may conclude that there is some natural connection between this meteor and magnetism. This should farther incite us to observe the circumstance formerly mentioned, *viz.* that the south end of the dipping needle points to that part of the heavens where the rays of the aurora appear to converge. We wish that this were diligently observed in places which have very different variation and dip of the mariner's needle.

For the diurnal and this irregular variation, consult the Dissertations of Celsius and of Hiorter, in the *Memoirs of Stockholm*; Wargentin, *Philosophical Transactions*, Vol. 48. Braun *Comment. Petropol. Novi*, T. V. VII. IX.; Graham and Canton as above.

TEMPERAMENT

OF THE

SCALE OF MUSIC.

328. When the considerate reader reflects on the large and almost numberless dissertations on this subject, by the most eminent philosophers, mathematicians, and artists, both of ancient and modern times, and the important points which divided, and still divide, their opinions, he will not surely expect, in a Work like this, the decision of a question which has hitherto eluded their researches. He will rather be disposed, perhaps, to wonder how a subject of this nature ever acquired such importance in the minds of persons of such acknowledged talents as Pythagoras, Aristotle, Euclid, Ptolemy, Galileo, Wallis, Euler, and many others, who have written elaborate treatises on the subject; and his surprise will increase, when he knows that the treatises on the scale of music are as numerous and voluminous in China, without any appearance of their being borrowed from the ingenious and speculative Greeks.

The ingenious, in all cultivated nations, have remarked the great influence of music; and they found no difficulty in persuading the nations that it was a gift of the gods. Apollo and his sacred choir are perhaps the most respectable inhabitants of the mythological heavens of the Greeks. Therefore all nations have considered music as a proper part of their religious worship. We doubt not but that they found it fit for exciting or supporting those emotions and sentiments which were suited to adoration, thanks, or petition. Nor would the Greeks have admitted music into their serious dramas, if they had not perceived that it heightened the effect. The same experience made them employ it as an aid to military enthusiasm; and it is recorded as one of the respectable accomplishments of Epaminondas, that he had the musical instructions of the first masters, and was eminent as a performer.

Thus was the study of music ennobled, and recommended to the attention of the greatest philosophers. Its cultivation was held an object of national concern, and its professors were not allowed to corrupt it in order to gratify the fastidious taste of the luxurious or the sensualist, who sought from it nothing but amusement. But its influence was not confined to these public purposes; and, while the men of speculation found in music an inexhaustible fund of employment for their genius and penetration, and their poets felt its aid in their compositions, it was hailed by persons of all ranks as the soother of the cares and anxieties, and sweetener of the labours of life. *O Phœbe decus!—laborum dulce lenimen.*

329. It is chiefly in this humble department of musical influence that we propose at present to lend our aid.

To be able to tune a harpsichord with certainty and accuracy, seems an indispensible qualification of any person worthy of the name of a *musician.* It would certainly be thought an unpardonable deficiency in a violin performer if he could not tune his instrument; yet we are well inform-

ed, that many professional performers on the harpsichord cannot do it, or cannot do it any other way than by uncertain and painful trial, and, as it were, groping in the dark; and that the tuning of harpsichords and organs is committed entirely to tuners by profession. This is a great inconvenience to persons residing in the country; and therefore many take lessons from the professed harpsichord tuners, who also profess to teach this art. We have been present during some of these lessons; but it did not appear to us that the instructions were such as could enable the scholar to tune an instrument when alone, unless the lessons had been so frequent as to form the ear to an instantaneous judgement of tune by the same habit that had instructed the teacher. There seemed to be little principle that could be treasured up and recollected when wanted.

330. Yet we cannot help thinking that there are phenomena of facts in music, sufficiently precise to furnish principles of absolute certainty for enabling us to produce temperaments of the scale which shall have determined characters, and among which we may choose such a one as shall be preferable to the others, according to the purposes we have in view; and we think that these principles are of such easy application, that any person, of a moderate sensibility to just intonation, may, without much knowledge or practice in music, tune his harpsichord with all desirable accuracy. We propose to lay these before the reader. We might content ourselves with simply giving the practical rules deduced from the principles; but it is surely more desirable to perceive the validity of the principles. This will give us confidence in the deduced rules of practice.

331. It is a most remarkable fact, that, in all nations, however they may differ in the structure of that chaunt which we call the accent, or tone, or twang, in the colloquial language of a particular nation, or in the favourite phrases or passages which are most frequent in their songs, all men make use of the same rises and falls, or inflections of voice

in their musical language or airs. We have heard the songs of the Iroquois, the Cherokee, and the Esquimaux, of the Carib, and the inhabitant of Paraguay; of the African of Negroland and of the Cape, and of the Hindoo, the Malay, and the native of Otaheite—and we found none that made use of a different scale from our own, although several seemed to be very sorry performers by any scale. There must be some natural foundation for this uniformity. We may never discover this; but we may be fortunate enough to discover facts in the phenomena of sound which invariably accompany certain modifications of musical sentiment. If we succeed, we are entitled to suppose that such inseparable companions are naturally connected; and to conclude, that if we can insure the appearance of those facts in sound, we shall also give occasion to those musical sentiments or impressions.

332. There is a quality in lengthened or continued sound which we call its pitch or note, by which it may be accounted shrill or hoarse. It may be very hoarse in the beginning, and during its continuance it may grow more and more shrill by imperceptible gradations. In this case we are sensible of a kind of progress from the one state of sound to the other. Thus, while we gently draw the bow across the string of a bass viol, if we at the same time slide the finger slowly along the string, from the nut toward the bridge, the sound, from being hoarse, becomes gradually acute or shrill. Hoarse and shrill therefore are not different qualities, although they have different names, but are different states or degrees of the same quality, like cold and heat, near and far, early and late, or, what is common to all these, little and great. A certain state of the air is accounted neither hot nor cold. All states on one side of this are called warm or hot; and all on the other are cold. In like manner, a certain sound is the boundary between those that are called hoarse and those called shrill. The chemist is accustomed to say, that the temperature of a body is higher when it is

warmer, and lower when colder. In like manner, we are accustomed to say, that a person raises or depresses the pitch of his voice when it becomes more shrill or more hoarse. The antient Greeks, however, called the shriller sounds *low* and the hoarser sounds *high*; probably because the hoarser sounds are generally stronger or louder, which we are also accustomed to consider as higher. In common language, a low pitch of voice means a faint sound, but in musical language it means a hoarser sound. The sound that is neither hoarse nor shrill is some ordinary pitch of voice, but without any precise criterion.

333. The change observed in the pitch of a violin string, when the finger is carried along the finger-board with a continued motion, is also continuous; that is, not by starts: we call it gradual, for want of a better term, although gradual properly means *gradatim*, by degrees, steps, or starts, which are not to be distinguished in this experiment. But we may make the experiment in another way. After sounding the open string, and while the bow is yet moving across it, we may put down the finger about $1\frac{2}{3}$ inches from the nut. This will change the sound into one which is *sensibly* shriller than the former, and there is a manifest start from the one to the other. Or we may put down the finger $2\frac{1}{2}$ inches from the nut; the sound of the open string will change to a shriller sound, and we are sensible that this change or step is greater than the former. Moreover, we may, while drawing the bow across the string, put down one finger at $1\frac{2}{3}$ inches, and, immediately after, put down another finger at $2\frac{1}{2}$ inches from the nut. We shall have three sounds in succession, each more shrill than the preceding, with two manifest steps, or subsultory changes of pitch.

334. Now since the last sound is the same as if the second had not been sounded, we must conceive the sum of the two successive changes as equivalent or equal to the change from the first to the third This change seems somehow to

include the other two, and to be made up of them, as a whole is made up of its parts, or as $2\frac{1}{2}$ inches are made up of $1\frac{2}{3}$ and $\frac{5}{6}$ of an inch, or as the sum 15 is made up of 10 and 5.

335. Thus it happens that thinking persons conceive something like or analogous to a distance, or interval, between these sounds. It is plain, however, that there can be no real distance or space interposed between them; and it is not easy to acquire a distinct notion of the bulk or magnitude of these intervals. This conception is purely figurative and analogical; but the analogy is very good, and the observation of it, or conjecture about it, has been of great service in the science of music, by making us search for some precise measure of those manifest intervals of musical sounds.

336. It must now be remarked, that it is in this respect alone that sounds are susceptible of music. Nor are all sounds possessed of this quality. The smack of a whip, the explosion of a musket, the rushing of water or wind, the scream of some animals, and many other sounds, both momentary and continuous, are mere noises; and can neither be called hoarse nor shrill. But, on the other hand, many sounds, which differ in a thousand circumstances of loudness, smoothness, mellowness, &c. which make them pleasant or disagreeable, have this quality of musical pitch, and may thus may be compared. The voice of a man or woman, the sound of a pipe, a bell, a string, the voice of an animal, nay the single blow on an empty cask—may all have one pitch, or we may be sensible of the interval between them. We can, in all cases, tighten or slacken the string of a violin, till the most uninformed hearer can pronounce with certainty that the pitch is the same. We are indebted to the celebrated Galileo for the discovery of that physical circumstance in all those sounds which communicates this remarkable quality to them, and even enables us to induce it on any noise whatever, and to determine, with

the utmost precision, the musical pitch of the sound, and the interval between any two such sounds. Of this we shall speak fully hereafter; and at present we only observe, that two sounds, having the same pitch, are called UNISONS by musicians, or are said to be in *unison* to one another.

337. When two untaught men attempt to sing the same air together, they always sing in unison, unless they expressly mean to sing in different pitches of voice. Nay, it is an extremely difficult thing to do otherwise, except in a few very peculiar cases. Also, when a man and woman, wholly uninstructed in music, attempt to sing the same air, they also *mean* to sing the same musical notes through the whole air; and they generally imagine that they do so. But there is a manifest difference in the sounds which they utter, and the woman is said to sing more SHRILL, and the man more HOARSE. A very plain experiment, however, will convince them that they are mistaken. *N. B.* We are now supposing that the performers have so much of a musical ear, and flexible voice, as to be able to sing a common ballad, or a psalm tune, with tolerable exactness, and that they can prolong or dwell upon any particular note when desired.

Let them sing the common psalm tune called St. David's in the same way that they practice at church; and when they have done it two or three times, in order to fix their voices in tune, and to feel the general impression of the tune, let the woman hold on in the first note of the tune, which we suppose to be g, while the man sings the first three in succession, namely g, d, $\overline{g}$. He will now perceive, that the last note sung by himself is the same with that sung by the woman, and which she thinks that she is still holding on in the first note of the tune. Let this be repeated till the performance becomes easy. They will then perceive the perfect sameness, in respect of musical pitch, of the woman's first note of this tune and the man's third note. Some difference however, will still be perceived; but it will not be in the

pitch, but in the smoothness, or clearness, or other agreeable quality of the woman's note.

338. When this is plainly perceived, let the man try by what continued steps he must raise his pitch, in order to arrive at the woman's note from his own. If he has been accustomed to common ballad singing, he will have no great difficulty in doing this; and will find that, beginning with his own note, and singing, gradually up, his eighth note will be the woman's note. In short, if two flutes be taken, one of which is twice as long as the other, and if the man sing in unison with the large flute, the woman, while singing, as she thinks, the same notes with the man, will be found to be singing in unison with the smaller flute.

339. This is a remarkable and most important fact in the phenomena of music. This interval, comprehending and made up of seven smaller intervals, and requiring eight sounds to mark its steps, is therefore called an OCTAVE. Now, since the female performer follows the same dictates of natural ear in singing her tune that the man follows in singing his, and all hearers are sensible that they are singing the same tune, it necessarily follows, that the two scrieses of notes are perfectly similar, though not the same: For there must be the same interval of an octave between any step of the lower octave and the same step of the upper one. In whatever way, therefore, we conceive one of these octaves to be parcelled out by the different steps, the partition of both must be similar. If we represent both by lines, these lines must be similarly divided. Each partial interval of the one must bear the same relation to the whole, or to any other interval, as its similar interval in the other octave bears to the whole of that octave, or to the other corresponding interval in it.

340 Farther, we must now observe, that although this similarity of the octaves was first observed or discovered by means of the ordinary voices of man and woman, and is a legitimate inference from the perfect satisfaction that each

feels in singing what they think the same notes, this is not the only foundation or proof of the similarity. Having acquired the knowledge of that physical circumstance, on which the pitch of musical sounds depends, we can demonstrate, with all the rigour of geometry, that the several notes in the man and woman's octave *must* have the same relation to their respective commencements, and that these two great intervals are similarly divided. But farther still, we can demonstrate that this similarity is not confined to these two octaves. This may even be proved, to a certain extent, by the same original experiment. Many men can sing two octaves in succession, and there are some rare examples of persons who can sing three. This is more common in the female voice. This being the case, it is plain that there will be two octaves common to both voices; and therefore four octaves in succession, all similar to each other. The same similarity may be observed in the sounds of instruments which differ only by an octave. And thus we demonstrate that all octaves are similar to each other. This similarity does not consist merely in the similarity of its division. The sound of a note and its octave are so like each other, that if the strength or loudness be properly adjusted, and there be no difference in kind, or other circumstances of clearness, smoothness, &c. the two notes, when sounded together, are indistinguishable, and appear only like a more brilliant note. They coalesce into one sound. Nay, most clear mellow notes, such as those of a fine human voice, really contain each two notes, one of which is octave to the other.

341. We said that this resemblance of octaves is an important fact in the science of music. We now see why it is so. The whole scale of music is contained in one octave, and all the rest are only repetitions of this scale. And thus is the doctrine of the scale of melody brought within a very moderate compass, and the problem is reduced to that of the repartition of a single octave, and some attention to the

junction with the similar scales of the adjoining octaves. This partition is now to be the subject of discussion.

342. In the infancy of society and cultivation, it is probable that the melodies or tunes, which delighted the simple inhabitants, were equally simple. Being the spontaneous effusions of individuals, perhaps only occasional, and never repeated, they would perish as fast as produced. The airs were probably connected with some of the rude rhimes, or gingles of words, which were bandied about at their festivals; or they were associated with dancing. In all these cases they must have been very short, consisting of a few favourite passages or musical phrases. This is the case with the common airs of all simple people to this day. They seldom extend beyond a short stanza of poetry, or a short movement of dancing. The artist who could compose and keep in mind a piece of considerable length, must have been a great rarity, and a minstrel fit for the entertainment of princes; and therefore much admired, and highly rewarded: his excellencies were almost incommunicable, and could not be preserved in any other way but by repeated performance to an attentive hearer, who must also be an artist, and must patiently listen, and try to imitate; or, in short, to get the tune by heart. It must have been a long time before any distinct notion was formed of the relation of the notes to each other. It was perhaps impossible to recollect today the precise notes of yesterday. There was nothing in which they were fixed till instrumental music was invented. This has been found in all nations; but it appears that long continued cultivation is necessary for raising this from a very simple and imperfect state. The most refined instrument of the Greek musicians was very far below our very ordinary instruments. And, till some method of notation was invented, we can scarcely conceive how any determined partition of the octave could be made generally known.

343. Accordingly, we find that it was not till after a long while, and by very rude and awkward steps, that the Greeks

perceived that the whole of music was comprised in the octave. The first improved lyre had but four strings, and was therefore called a TETRACHORD; and the first flutes had but three holes, and four notes; and when more were added to the scale, it was done by joining two lyres and two flutes together. Even this is an instructive step in the history of musical science: For the four sounds of the instrument have a natural system, and the awkward and groping attempts to extend the music, by joining two instruments, the scale of the one following, or being a continuation of that of the other, pointed out the DIAPASON or *totality* of the octave, and the relation of the whole to a principal sound, which we now call the *fundamental* or *key*, it being the lowest note of our scale, and the one to which the other notes bear a continual reference. It would far exceed the limits of this work to narrate the successive changes and additions made by the Greeks in their lyre; yet would this be a very sure way of learning the natural formation of our musical scale. We must refer our readers to Dr. Wallis's Appendix to his edition of the Commentary of Porphyrius in Ptolemy's Harmonics, as by far the most perspicuous account that is extant of the Greek music. We shall pick out from among their different attempts such plain observations as will be obvious to the feelings of any person who can sing a common tune.

344. Let such a person first sing over some plain and cheerful, or at least not mournful, tune, several times, so as to retain a lasting impression of the chief note of the tune, which is generally the last. Then let him begin, on the same note, to sing in succession the rising steps of the scale, pronouncing the syllables *do, re, mi, fa, sol, la, si, do.* He will perhaps observe, that this chaunt naturally divides itself into two parts or phrases, as the musicians term it. If he does not, of himself, make this remark, let him sing it, however, in that manner, pausing a little after the note *fa*. Thus, *do, re, mi, fa; sol, la, si, do.—Do, re, mi, fa; sol, la, si, do.*

Having done this several times, and then repeated it without a pause, he will become very sensible of the propriety of the pause, and of this natural division of the octave. He will even observe a considerable similarity between these two musical phrases, without being able, at first, to say in what it consists.

345. Let him now study each phrase apart, and try to compare the magnitude of the changes of sound; or steps which he makes in rising from *do* to *re*, from *re* to *mi*, and from *mi* to *fa*. We apprehend that he will have no difficulty in perceiving, after a few trials, that the steps *do re*, and *re mi*, are sensibly greater than the step *mi fa*. We feel the last step as a sort of slide; as an attempt to make as little change of pitch as we can. Once this is perceived, it will never be forgotten. This will be still more clearly perceived, if, instead of these syllables, he use only the vowel *a*, pronounced as in the word *hall*, and if he sing the steps, sliding or slurring from the one to the other. Taking this method he cannot fail to notice the smallness of the third step

346. Let the singer farther consider, whether he does not feel this phrase musical or agreeable, making a sort of tune or chaunt, and ending or closing agreeably after this slide of a small, or, as it were, half step. It is generally thought so; and is therefore called a CLOSE, a CADENCE, when we end with a half step ascending.

347. Let the singer now resume the whole scale, singing the four last notes *sol*, *la*, *si*, *do*, louder than the other four, and calling off his attention from the low phrase, and fixing it on the upper one. He will now be able to perceive that this, like the other, has two considerable steps; namely, *sol*, *la*, and *la*, *si*, and then a smaller step, *si*, *do*. A few repetitions will make this clear, and he will then be sensible of the nature of the similarity between these two phrases, and the propriety of this great division of the scale into the intervals *do*, *fa*, and *sol*, *do*, with an interval *fa*, *sol*, between them

This was the foundation of the tetrachords, or lyres of four strings, of the Greeks. Their earliest music or modulation seems to have extended no farther than this phrase. It pleased them, as a ring of four bells pleases many country parishes.

348. The singer will perceive the same satisfaction with the close of this second phrase as with that of the former: and if he now sing them both, in immediate succession, with a slight pause between, we imagine that he will think the close or cadence on the upper *do* even more satisfactory than that on the *fa*. It seems to us to complete a tune. And this impression will be greatly heightened, if another person, or an instrument, should sound the lower *do*, while he closes on the upper *do* its octave. *Do* seems to be expected, or looked for, or sought after. We take *si* as a step to *do*, and there we rest.

349 Thus does the octave appear to be naturally composed of seven steps, of which the first, second, fourth, fifth, and sixth, are more considerable, and the third and seventh very sensibly smaller. Having no direct measures of their quantity, nor even a very distinct notion of what we mean by their quantity, magnitude, or bulk, we cannot pronounce, with any certainty, whether the greater steps are equal or unequal; and we presume them to be equal. Nor have we any distinct notion of the proportion between the larger and smaller steps. In a loose way we call them half notes, or suppose the rise from *mi* to *fa*, or from *si* to *do*, to be one-half of that from *do* to *re*, or from *re* to *mi*.

350. Accordingly, this seems to have been all the musical science attained by the Greek artists, or those who did not profess to speak philosophically on the subject. And even after Pythagoras published the discovery which he had made, or more probably had picked up among the Chaldeans or Egyptians, by which it appeared, that accurate measures of sounds, in respect to gravity and acuteness, were attainable, it was affirmed by Aristoxenus, a scholar of Aristotle, and other eminent philosophers, that these measures were alto-

gether artificial, had no connection with music, and that the ear alone was the judge of musical intervals. The artist had no other guide in tuning his instrument; because the ratios, which were said to be inherent in the sounds (though no person could say how), were never perceived by the ear. The justice of this opinion is abundantly confirmed by the awkward attempt of the Greeks to improve the lyre by means of these boasted ratios. Instead of illustrating the subject, they seem rather to have brought an additional obscurity upon it, and threw it into such confusion, that although many voluminous dissertations were written on it, and on the composition of their musical scale, the account is so perplexed and confused, that the first mathematicians and artists of Europe acknowledged, that the whole is an impenetrable mystery. Had the philosophers never meddled with it; had they allowed the practical musicians to construct and tune their instruments in their own way, so as to please their ear, it is scarcely possible that they should not have hit on what they wanted, without all the embarrassment of the chromatic and enharmonic scales of the lyre. It is scarcely possible to contrive a more cumbersome method of extending the simple scale of Nature to every case that could occur in their musical compositions, than what arose from the employment of the musical ratios. This seems a bold assertion; but we apprehend that it will appear to be just as we proceed.

351. The practical musicians could not be long of finding the want of something more than the mere diatonic scale of their instruments. As they were always accompanied by the voice, it would often happen that a lyre or flute, perfectly tuned, was too low or too high for the voice that was to accompany it. A singer can pitch his tune on any sound as a key; and if this be too high for the singer who is to accompany him, he can take it on a lower note. But a lyrist cannot do this. Suppose his instrument two notes too low, and that his accompanyist can only sing it on the key which

is the *si* of the lyre. Should the lyrist begin it on that key, his very first step is wrong, being but a half step, whereas it should be a whole one. In short, all the steps but one will be found wrong, and the lyrist and singer will be perpetually jarring. This is an evident consequence of the inequality of the fourth and seventh steps to the rest. And if the other steps, which we imagine to be equal, be not exactly so, the discordance will be still greater.

The method of remedying this is very obvious. If the intervals *mi fa* and *si do*, are half notes, we need only to interpose other sounds in the middle between each of the whole notes: and then, in place of seven unequal steps, we shall have twelve equal ones, or twelve intervals, each of them equal to a semitone. The lyre thus constructed will now suit any voice whatever. It will perfectly resemble our keyed instruments, the harpsichord, or organ, which have twelve seemingly equal intervals in the octave. Accordingly, it appears that such additions were practised by the musicians of Greece, and approved of by Aristoxenus, and by all those who referred every thing to the judgment of the ear. And we are confident that this method would have been adopted, if the philosophers had had less influence, and if the Greeks had not borrowed their religious ceremonies along with their musical science. Both of these came from the same quarter; they came united; and it was sacrilegious to attempt innovations. The doctrine of musical ratios was an occupation only for the refined, the philosophers; and by subjecting music to this mysterious science, it became mysterious also, and so much the more venerable. The philosophers saw, that there was in Nature a certain inscrutable connection between mathematical ratios and those intervals which the ear relished and required in melody: but they were ignorant of the nature and extent of this connection.

What is this connection, or what is meant when we speak of the ratios of sounds! Simply this:—Pythagoras is said to have found, that if two musical cords be strained by equal

weights, and one of them be twice the length of the other, the short one will sound the octave to the note of the other. If it be two-thirds of the length of the long string, it will sound the fifth to it. If the long string sound *do*, the short one will sound *sol*. If it be three-fourths of the length, it will sound the fourth or *fa*. Thus the ratio of 2 : 1 was called the ratio of the DIAPASON; that of 3 : 2 was called the DIAPENTE; and that of 4 : 3 the DIATESSARON. Moreover, if we now take all the four strings, and make that which sounds the gravest note, and is the longest, twelve inches in length; the short or octave string must be six inches long, or one-half of twelve; the diapente must be eight inches, or two-thirds of twelve; and the diatessaron must be nine inches, which is three-fourths of twelve. If we now compare the diapente, not with the gravest string, but with the octave of six inches, we see that they are in the ratio of 4 to 3, or the ratio of diatessaron. And if we compare the diatessaron with the octave, we see that their ratio is that of 9 : 6, or of 3 : 2, or the ratio of diapente. Thus is the octave divided into a fifth and a fourth, *do sol*, and *sol do*, in succession. Also the fourth *do fa*, and the fifth *fa do*, make up the octave. The note which stands as a fifth to one of the extreme sounds of the octave, stands as a fourth to the other. And, lastly, the two fourths *do fa*, and *sol do*, leave an interval, *fa sol* between them; which is also determined by nature, and the ratio corresponding to it is evidently that of 9 to 8.

352. This is all that was known of the connection of music with mathematical ratios. It is indeed said by Iamblichus, that Pythagoras did not make this discovery by means of strings, but by the sounds made by the hammers on the anvil in a smith's shop. He observed the sounds to be the key, the diatessaron, and the diapente of music; and he found, that the weights of the hammers were in this proportion; and as soon as he went home, he tried the sounds made by cords, when weights, in the proportions above-mentioned, were appended to them. But the whole story has the

air of a fable, and of ignorance. The sounds given by a smith's anvil have little or no dependance on the weight of the hammers; and the weights which are in the proportions of the numbers mentioned above will by no means produce the sounds alleged. It requires *four* times the weight to make a string round the octave, and *twice and a quarter* will produce the diapente, and *once and seven-ninths* will produce the diatessaron. It is plain, therefore, that they knew not of what they were speaking: yet, on this slight foundation, they erected a vast fabric of speculation; and in the course of their researches, these ratios were found to contain all that was excellent. The attributes of the Divinity, the symmetry of the universe, and the principles of morality, were all resolvable into the harmonic ratios.

353. In the attempts to explain, by means of the mysterious properties of the ratios 2 : 1, 3 : 2, 4 : 3, and 9 : 8, which were thus defined by Nature, it was observed, that their favourite lyres of four strings could be combined in two principal manners, so as to produce an extensive scale. One lyre may contain the notes *do, re, mi, fa;* and the acuter lyre may contain the notes *sol, la, si, do;* and, being set in succession, having the interval *fa sol* between the highest note of the one and the lowest of the other, they make a complete octave. These were called *disjoined tetrachords.* Again, a third tetrachord may be joined with the upper tetrachord last mentioned, in such sort, that the lowest note of the third tetrachord may be the same with the highest of the second. These were called *conjoined* tetrachords*.

354. By thus considering the scale as made up of tetrachords, the tuning of the lyre was reduced to great simpli-

* This is the *principle*, but not the precise *form*, of the disjoined and conjunct tetrachords. The Greeks did not begin the tetrachord with what we make the first note of our chaunt of four notes, but began one of them with *mi*, and the other with *si;* to which they afterwards added a note below. This beginning seems to have been directed by some of their favourite cadences; but it would be tedious to explain it.

city. The musician had only to make himself perfect in the short chaunt *do, re, mi, fa*, or to get it by heart, and to sing it exactly. This intonation would apply equally to the other *sol, la, si, do*. We are well informed that this was really the practice. The directions given by Aristoxenus, Nicanor, and others, for varying the tuning, according to certain occasional accommodations, shew distinctly that they did not tune as we do, sounding the two strings together, except in the case of the diapason or octave. It was all done by the judgment of the ear in melody. The most valuable circumstance in the discovery of Pythagoras was the determination of the interval between the fourth and the fifth, by which the tetrachords were separated. The filling up of each tetrachord was left entirely to the ear; and when the doctrine of the mathematical ratios shewed that the large intervals *do re, re mi, fa sol, sol la, la si*, should not be precisely equal, Aristoxenus refused the authority of the reasons alleged for this inequality, because the ear perceived none of the ratios as ratios, and could judge only of sounds. He farther asserted, that the inequalities which the Pythagoreans enjoined, were so trifling, that no ear could possibly perceive them. And accordingly, the theorists disputed about the respective situations of the greater and smaller tones (so they named the great steps) so much spoken of, and had different systems on the subject.

355. But the strongest proof of the indistinct notion that the theorists entertained about the influence of these ratios in music is, that they would admit no more but those introduced by Pythagoras; and their reasons for the rejection of the ratio of 5 to 4, and of 6 to 5, were either the most whimsical fancies about the perfections of the sacred ratios, or assumptions expressly founded on the supposition, that the ear perceives and judges of the ratios as ratios; than which nothing can be more false. Had they admitted the ratio of 5 to 4, they would have obtained the third note of the scale, and would at once have gotten the whole scale of our

music. The ratios of 6 : 5, and 16 : 15, follow of course; and every sound of the tetrachords would have been determined. For 5 : 4 being the ratio of the major third, which is perfectly pleasing to the ear, as the *mi* to the note *do*, and 3 : 2 being the ratio of the fifth *do sol*, there is another interval *mi sol* determined; and this ratio, being the difference between *do sol* and *do mi*, or between 3 : 2 and 5 : 4, is evidently 6 : 5. In like manner, the interval *mi fa* is determined, and its ratio, being 4 : 3—5 : 4, is 16 : 15.

But farther; we shall find, upon trial, that if we put in a sound above *sol*, having the relation 5 : 4 to *fa*, it will be perfectly satisfactory to the ear if sung as the note *la*. And if, in like manner, we put in a note above *la*, having the relation 5 : 4 to *sol*, we find it satisfactory to the ear when used as *si*. If we now examine the ratios of these artificial notes, we shall find the ratio of the notes *sol la* to be 10 : 9, and that of *la si* to be 9 : 8, the same with that *fa sol*; also *si do* will appear to be 16 : 15, like that of *mi fa*.

We have no remains of the music of the Greeks, by which we can learn what were their favourite passages or musical phrases; and we cannot see what caused them to prefer the fourth to the major third. Few musicians of our times think the fourth in any degree comparable with the major third for melodiousness, and still fewer for harmoniousness. The piece or tune published by Kircher from Alypius is very suspicious, as no other person has seen the MS.; and the collection found at Buda is too much disfigured, and probably of too late a date, to give us any solid help. In all probability, the common melodies of the Greeks abounded in easy leaps up and down on the third and fifth, and on the fourth and sixth, just as we observe in the airs for dancing among all simple people. Their accomplished performers had certainly great powers both of invention and execution; and the chromatic and enharmonic divisions of the scale were certainly practised by them, and not merely the speculations of mathematicians. To us, the enharmonic scale appears the

ost jarring discord ; but this is certainly owing to our not eing any pieces of the music so composed, and because we nnot in the least judge by harmony what the effect of enrmonic melody would be. But we have sufficient evidence, om the writings of the ancient Greeks, that the enharmonic usic fell into disuse even before the time of Ptolemy, and as totally and irrecoverably lost before the 5th century. ven the chromatic was little practised, and was chiefly emoyed for extending the common scale to keys which were ldom used. The uncertainties respecting even the comon scale remained the same as ever ; and although Ptoley gives (among others) the very same that is now admitd as the only perfect one, namely, his *diatonicum intensum*, s reasons of preference, though good, are not urged with rong marks of his confidence in them, nor do they seem to ave prevailed.

356 These observations shew clearly, that the perception melody alone is not sufficiently precise for enabling us to equire exact conceptions of the scale of music. The whole the practicable science of the ancients seems to amount to o more than this, that the octave contained five greater and vo smaller intervals, which the voice employed, and the ar relished. The greater intervals seemed all of one magitude ; and the smaller intervals appeared also equal, but ne ear cannot judge what proportion they bear to the larger nes. The musicians thought them larger than one-half of ne great intervals (and indeed the ratio 16 : 15 of the artcial *mi fa* and *si do*, is greater than the half of 9 : 8 or 0: 9). Therefore they allowed the theorists to call them *limas* instead of *hemitones ;* but they, as well as the theorists, iffered exceedingly in the magnitudes which they assigned hem.

357. The best way that we can think of for expressing the cale of the octave is, by dividing the circumference of a cirle in the points C, D, E, F, G, A, and B, (Plate V. fig. 1.), n the proportion we think most suitable to the natural

scale of melody. According to the practical notion n
under our consideration, the arches CD, DE, FG, GA, a
AB, are equal, containing nearly 59° ; and the arches
and BC are also equal, but smaller than the others, c
taining about $33\frac{1}{2}$. Now, suppose another circle, on a pi
of card paper, divided in the same manner, to move rou
their common centre, but instead of having its points of d
sion marked C, D, E, &c. let them be marked *do*, *re*, *mi*,
sol, *la*, *si*. It is plain, that to whatever point of the ou
circle we set the point *do* of the inner one, the other poi
of the outer circle will shew the common notes which are
for those steps of the scale. The similarity of all octa
makes this simple octave equivalent to a rectilineal scale
milarly divided, and repeated as often as we please. (Pl
V. fig. 1.) represents this instrument, and will be often
ferred to. A sort of symmetry may be observed in it. T
point D seems to occupy the middle of the scale, and *re* see
to be the middle note of the octave. The opposite arch G
and the corresponding interval *sol la*, seems to be the mid
interval of the octave. The other notes and intervals
similarly disposed on each side of these. This circumsta
seems to have been observed by the Greeks, by the inha
tants of India, by the Chinese, and even by the Mexica
The note *re*, and the interval *sol la*, have gotten distingui
ed situations in their instruments and scales of music.

358. With respect to the division of the circles, we sh
only observe at present, that the dotted lines are confo
able to the principles of Aristoxenus, the whole octave
ing portioned out into five larger and equal intervals,
two smaller, also equal. The larger are called *mean* or
dium tones ; and the smaller are called *limmas* or *semito*
The full lines, to which the letters and names are affixed,
vide the octaves into the artificial portions, determined
means of the musical ratios, the arches being made p
portional to the measures of those ratios. Thus the arc
CD, FG, AB, are proportional to the measure or logarit

of the ratio 9 : 8; GA and DE are proportional to the logarithm of 10 : 9; and the arches EF and BC are proportional to the logarithm of 16 : 15. We have already mentioned the way in which those ratios were applied, and the authority on which they were selected. We shall have occasion to return to this again. The only farther remark that is to be made with propriety in this place is, that the division on the Aristoxenean principles, which is expressed in this figure, is one of an indefinite number of the same kind. The only principle adopted in it is, that there shall be five mean tones, and two small equal semitones; but the magnitude of these is arbitrary. We have chosen such, that two mean tones are exactly equal to the arch CE, determined by the ratio 5 : 4. The reasons for this preference will appear as we proceed*.

By this little instrument (the invention, we believe, of a Mr. D'Ormisson, about the beginning of last century), we see clearly the insufficiency of the seven notes of the octave for performing music on different keys. Set the flower de luce at the Aristoxenean B, and we shall see that E is the only note of our lyre which will do for one of the steps of the octave in which we intend to sing and accompany. We have no sounds in the lyre for *re*, *mi*, *sol*, *la*, *si*. The remedy is as clearly pointed out. Let a set of strings be made, having the same relation to *si* which those of the present lyre have to *do*, and insert them in the places pointed out by the Aristoxenean divisions of the moveable octave. We need only five of them, because the *si* and *fa* of the present lyre will answer. These new sounds are marked by a +.

359. But it was soon found, that these new notes gave but indifferent melody, and that either the ear could not determine the equality of the tones and semitones exactly

* We shall be abundantly exact, if we make CD = 61°,72; CE = 115°,9; CF = 149°,42; CG = 210°,58; CA = 265°,3; and CB = 326°,48.

enough, or that no such partition of the octave would answer. The Pythagoreans, or partisans of the musical ratios, had told them this before. But they were in no better condition themselves; for they found, that if a series of sounds, in perfect relation to the octave, be inserted in the manner proposed, the melody will be no better. They put the matter to a very fair trial. It is easy to see, that no system of mean tones and limmas will give the same music on every key, unless the tones be increased, and the limmas diminished, till the limma becomes just half a tone. Then all the intervals will be perfectly equal. The mathematicians computed the ratios which would produce this equality, and desired the Aristoxeneans to pronounce on the music. It is said, that they allowed it to be very bad in all their most favourite passages. Nothing now remained to the Aristoxeneans but to attempt occasional methods of tuning. They saw clearly, that they were making the notes unequal which Nature made equal. The Pythagoreans, in like manner, pointed out many alterations or corrections of intervals which suited one tetrachord, or one part of the octave, but did not suit another. Both parties saw that they were obliged to deviate from what they thought natural and perfect; therefore they called these alterations of the natural or perfect scale *temperament*.

The accomplished performers were the best judges of the whole matter, and they derived very little assistance from the mathematicians: For although the rigid rules delivered by them be acknowledged to be perfectly exact, the execution of those rules is not susceptible of the same exactness. Their lyres are tuned, not by mathematical operations, but by the ear. It does not appear that they had musical instruments with divided finger-boards, like our bass viols and guitars; and even on these, it is well known that the pressure and touch of the finger may vary so much, that the most exact placing of the frets will not insure the nice degrees of the sounds. The flutes are the only instruments

of the ancients that are capable of accurate sounds. But flute-makers know very well, that they cannot be tuned by mathematical operations, but by the ear alone. This accounts for the great prices paid for a well-tuned flute. Some have cost L.700, and L.50 was a very common price.

360. Such seems to have been the state of the ancient music. There was little or no science in it. There was, indeed, a most abstruse and refined science coupled with it; but by a very slight connection: and it seems to have been nothing more than an amusement for the ingenious and speculative Greeks. Nor could it, in our opinion, be better, so long as they had no guide in tuning but the judgment of the ear in melody. Many writers insist that the Greeks had a knowledge of what *we* call *harmony* also. The word ἁρμονια is constantly used by them: but it does not mean what we call harmony, the pleasant coalescence of sumultaneous sounds. It comes from ἁρμος, or from ἁρμοζω, and signifies *aptitude*, *fitness*, and would, in general, be better translated by *symmetry*. But we cannot conceive that they paid any marked attention to the effect of simultaneous sounds, so as to enjoy the pleasure of certain consonances, and employ them in their compositions. We judge in this way from the rank which they gave them in their scale. To prefer the fourth to the major third seems to us to be impossible, if it be meant of simultaneous sounds. And the reason which is assigned for the preference can have no value in the opinion of a musician. It is because the ratio of 4 : 3 is simpler than that of 5 : 4. For the same reason, the fifth is preferred to both, and the octave to all the three, and unison to every other consonance. They would not allow the major third 5 : 4 to be a concord at all. We have made numberless trials of the different concords with persons altogether ignorant of music. We never saw an instance of one who thought that mere unison gave any positive pleasure. None of all whom we examined had much pleasure from an octave. All, without exception, were delighted with a fifth, and with

a major third; and many of them preferred the latter. All of them agreed in calling the pleasure from the fifth a *sweetness*, and that from the major third a *cheerfulness*, or *smartness*, or by names of similar import. The greater part preferred even the major sixth to the fourth, and some felt no pleasure at all from the fourth Few had much pleasure from the minor third or minor sixth. *N. B.* Care was taken to sound these concords without any preparation—merely as sounds—but not as making part of any musical passage. This circumstance has a great effect on the mind. When the minor third and sixth were heard as making part of the minor mode, all were delighted with it, and called it sweet and mournful. In like manner, the chord $\frac{6}{4}$ never failed to give pleasure. Nothing can be a stronger proof of the ignorance of the ancients of the pleasures of harmony.

361. We do not profess to know when this was discovered. We think it not unlikely that the Greeks and Italians got it from some of the northern nations whom they called *Barbarians*. We cannot otherwise account for its prevalence through the whole of the Russian empire—the ancient Slavi had little commerce with the empire of Rome or of Constantinople; yet they sung in parts in the most remote periods of their history of which we have any account; and to this day, the most uncultivated boor in the Russian empire would be ashamed to sing in unison. He listens a little while to a new tune, holding his chin to his breast; and as soon as he has got a notion of it, he bursts out in concert, throwing in the harmonic notes by a certain rule which he feels, but cannot explain. His harmonics are generally alternate major and minor thirds, and he seldom misses the proper cadences on the fifth and sixth key. Perhaps the invention of the organ produced the discovery. We know that this was as early as the second century*. It was hardly possible to

* It is said that the Chinese had an instrument of this kind long before the Europeans. Causeus says, that it was brought from China by a native, and was so small as to be carried in the hand. It is certain that the Emperor Con-

make much use of that instrument without perceiving the pleasure of concordant sounds.

362. The discovery of the pleasures of harmony occasioned a total change in the science of music. During the dark ages of Europe, it was cultivated chiefly by the monks: the organ was soon introduced into the churches, and the choral service was their chief and almost their only occupation. The very construction of this instrument must have contributed to the improvement of music, and instructed men in the nature of the scale. The pipes are all tuned by their lengths; and these lengths are in the ratios of the strings which give the same notes, when all are equally stretched. This must have revived the study of the musical ratios. The tuning of the organ was performed by consonance, and no longer depended on the nice judgment of sounds in succession. The dullest ear, even with total ignorance of music, can judge, without the smallest error, of an exact octave, fifth, third, or other concord; and a very mean musician could now tune an organ more accurately than Timotheus could tune his lyre. Other keyed instruments, resembling our harpsichord, were invented, and instruments with fretted finger boards. These soon supplanted the lyres and harps, being much more compendious, and allowing a much greater variety and rapidity of modulation. All these instruments were the fruits of harmony, in the modern sense of that word. The deficiencies of the old diatonic scale were now more apparent, and the necessity of a number of intercalary notes. The finger-board of an organ or harpsichord, running through a series of octaves, and admitting much more than the accompaniment of one note, pointed out new sources of musical pleasure arising from the fulness of the harmony; and, above all, the practice of choral singing suggested the

stantine Copronymus sent one to Pepin king of France in 757, and that his son Charlemagne got another from the Emperor Michael Paleologus. But they appear to have been known in the English churches before that time.

possibility of a pleasure altogether new. While a certain number of the choir performed the Cantus or Air of the music, it was irksome to the others to utter mere sounds, supporting or composing the harmony of the Cantus, without any melody or air in their own parts. It was thought probable that the harmonic notes might be so portioned out among the rest of the choir, that the succession of sounds uttered by each individual might also constitute a melody not unpleasant, and perhaps highly grateful. On trial it was found very practicable. Canons, motets, fugues, and other harmonies, were composed, where the airs performed by the different parts were not inferior in beauty to the principal. The notes which could not be thrown into this agreeable succession, were left to the organist, and by him thrown into the bass.

363. By all these practices, the imperfections of the scale of fixed sounds became every day more sensible, especially in full harmony. Scientific music, or the properties of the ratios, now recovered the high estimation in which they were held by the ancient theorists; and as the musicians were now very frequently men of letters, chiefly monks, of sober characters and decent manners, music again became a respectable study. The organist was generally a man of science, as well as a performer. At the first revival of learning in Europe, we find music studied and honoured with degrees in the universities, and very soon we have learned and excellent dissertations on the principles of the science. The inventions of Guido, and the dissertations of Salinas, Zarlino, and Xoni, are among the most valuable publications that are extant on music. The improvements introduced by Guido are founded on a very refined examination of the scale; and the temperaments proposed by the other two have scarcely been improved by any labours of modern date. Both these authors had studied the Greek writers with great care, and their improvements proceed on a complete knowledge of the doctrines of Pythagoras and Ptolemy.

364. At last the celebrated Galileo Galilei put the finishing hand to the doctrines of those ancient philosophers, by the discovery of the connection which subsists in nature between the ratios of numbers and the musical intervals of sounds. He discovered, that these numbers express the frequency of the recurring pulses or undulations of air which excite in us the sensation of sound. He demonstrated that if two strings, of the same matter and thickness, be stretched by equal weights, and be twanged or pinched so as to vibrate, the times of their vibrations will be as their lengths, and the frequency or number of oscillations made in a given time will be inversely as their lengths. The frequency of the sonorous undulations of the air is therefore inversely as the length of the string. When therefore we say that 2 : 1 is the ratio of the octave, we mean, that the undulations which produce the upper sound of this interval are twice as frequent as those which produce its fundamental sound. And the ratio 3 : 2 of the diapente or fifth, indicates that in the same time that the ear receives three undulations from the upper sound, it receives only two from the lower. Here we have a natural connection, not peculiar to the sounds produced by strings; for we are now able to demonstrate, that the sounds produced by bells are regulated by the same law. Nay, the improvements which have been made in the science of motion since the days of Galileo, shew us that the undulations of the air in pipes, where the air is the *only* substance moved, is regulated by the same law. It seems to be the general property of sounds which renders them susceptible of musical pitch, of acuteness, or gravity; and that a certain frequency of the sonorous undulations gives a determined and unalterable musical note. The writer of this article has verified this by many experiments. He finds, that *any noise whatever*, if repeated 240 times in a second, at equal intervals, produces the note C *sol fa ut* of the Guidonian gamut. If it be repeated 360 times, it produces the G *sol re ut*, &c. It was imagined, that only certain regular agitations of the

air, such as are produced by the tremor or vibration of elastic bodies, are fitted for exciting in us the sensation of a musical note. But he found, by the most distinct experiments, that any noise whatever will have the same effect, if repeated with due frequency, not less than 30 or 40 times in a second. Nothing surely can have less pretension to the name of a musical sound than the solitary snap which a quill makes when drawn from one tooth of a comb to another: but when the quill is held to the teeth of a wheel, whirling at such a rate, that 720 teeth pass under it in a second, the sound of *g in alt.* is heard most distinctly; and if the rate of the wheel's motion be varied in any proportion, the noise made by the quill is mixed in the most distinct manner with the musical note corresponding to the frequency of the snaps. The *kind* of the original noise determines the kind of the continuous sound produced by it, making it harsh and fretful, or smooth and mellow, according as the original noise is abrupt or gradual: but even the most abrupt noise produces a tolerably smooth sound when sufficiently frequent. Nothing can be more abrupt than the snap just now mentioned; yet the *g* produced by it has the smoothness of a bird's chirrup. An experiment was made, which was less promising of a sound than any that can be thought of. A stop-cock was so constructed, that it opened and shut the passage through a pipe 720 times in a second. This apparatus was fitted to the pipe of a conduit leading from the bellows to the wind-chest of an organ. The air was simply allowed to pass gently along this pipe by the opening of the cock. When this was repeated 720 times in a second, the sound *g in alt.* was most smoothly uttered, equal in sweetness to a clear female voice. When the frequency was reduced to 360, the sound was that of a clear but rather harsh man's voice. The cock was now altered in such a manner, that it never shut the hole entirely, but left about one third of it open. When this was repeated 720 times in a second, the sound was uncommonly smooth and sweet. When re-

duced to 360, the sound was more mellow than any man's voice at the same pitch. Various changes were made in the form of the cock, with the intention of rendering the primitive noise more analogous to that produced by a vibrating string. Sounds were produced which were pleasant in the extreme. The intelligent reader will see here an opening made to great additions to practical music, and the means of producing musical sounds, of which we have at present scarcely any conception; and this manner of producing them is attended with the peculiar advantage, that an instrument so constructed can never go out of tune in the smallest degree. But of this enough at present.

365. This discovery of Galileo's completed the Pythagorean theories, by supplying the only thing wanted for procuring confidence in them. We now see that the music of sounds depends on principles as certain and as plain as the elements of Euclid, and that every thing relating to the scale of music is attainable by mathematics. It is very true that we do not perceive the ratio 3 : 2 in the diapente, as having any relation to the numbers 3 and 2. But we perceive the sweetness of sound which characterises this concord. This is undoubtedly the perception of a certain physical fact involving this ratio, as much as the sweetness on our tongue is the perception of a certain manner of acting on the particles of sugar during their dissolution in the saliva.

The pleasure arising from certain consonances, such as *do sol*, is not more distinctly perceived than is the disagreeable feeling which other consonances produce, such as *do re*; and it was a fair field of disquisition to discover why the one pleased and the other displeased. We cannot say that this question has been completely decided. It has been ascribed to the coincidence of vibrations. In the octave, every second vibration of the treble note may be made to coincide with every vibration of the bass. But the pleasure arising from the different consonances does by no means follow the proportions of those coincidences of vibrations; for when two

notes are infinitely near to the state which would produce a complete coincidence, the actual coincidence is then exceedingly rare; and yet we know that such sounds yield very fine harmony. In tuning any concord, when the two notes are very discordant, the coinciding vibrations recur very frequently; and as we approach nearer and nearer to perfect concord, these coincidences become rarer and rarer; and if it be infinitely near to perfect concord, the coincidences of vibration will be infinitely distant from each other. This, and many other irrefragable arguments, demonstrate that coalescence of sound, which makes the pleasing harmony of a fifth, for example, does not arise from the coincidence of vibrations; and the only thing which we can demonstrate to obtain in all the cases where we enjoy this pleasure, is a certain arrangement of the component pulses, and a certain law of succession of the dislocations or intervals between the non-coinciding pulses. We are perfectly able to demonstrate that when, by continually screwing up one of the notes of a consonance, we render the real coincidence of pulses less frequent; the dislocations, or deviations from perfect coincidence, approach nearer and nearer to a certain defineable law of succession; and that this law obtains completely, when the perfect ratio of the duration of the pulse is attained, although perhaps at that time not one pulse of the one sound coincides with a pulse of the other. Suppose two organ pipes, sounding the note C *sol fa ut*, at the distance of ten feet from each other, and that their pulses begin and end at the same instant, making the most perfect coincidence of pulses—there is no doubt but that there will be the most perfect harmony: and we learn by experience that this harmony is perfectly the same, from whatever part of the room we hear it. This is an unquestionable fact. A person situated exactly in the middle between them will receive coincident pulses. But let him approach one foot nearer to one of the pipes, it is now demonstrable that the pulses, at their arrival at his ear, will be the most distant

from coincidence that is possible; for every pulse of one pipe will bisect the pulse from the other: but the law of succession of the deviations from coincidence will then obtain in the most perfect manner. A musical sound is the sensation of a certain form of the aerial undulation which agitates the auditory organ. The perception of harmonious sound is the sensation produced by another definite form of the agitation. This is the composition of two other agitations; but it is the compound agitation only that affects the ear, and it is its form or kind which determines the sensation, making it pleasant or unpleasant.

366. Our knowledge of mechanics enables us to describe this form, and every circumstance in which one agitation can differ from another, and to discover general features or circumstances of resemblance, which, in fact, accompany all perceptions of harmony. We are surely entitled to say that these circumstances are sure tests of harmony; and that when we have insured their presence, we have insured the hearing of harmony in the adjusted sounds. We can even go farther in some cases: We can explain some appearances which accompany imperfect harmony, and perceive the connection between certain distinct results of imperfect coincidences, and the magnitude of the deviations from perfect harmony which are then heard. Thus, we can make use of these phenomena, in order to ascertain and measure those deviations; and if any rules of temperament should require a certain determinate deviation from perfect harmony in the tuning of an instrument, we can secure the appearance of that phenomenon which corresponds to the deviation, and thus can produce the precise temperament suggested by our rules. We can, for example, destroy the perfect harmony of the fifth C g, and flatten the note g till it deviates from a perfect fifth in the exact ratio of 320 to 321, which the musicians call the one-fourth of a comma. The most exquisite ear for melody is almost insensible of a deviation four times greater than this; and yet a person who has no musical ear

at all, can execute this temperament by the rules of harmony without the error of the fortieth part of a comma.

367. For this most valuable piece of knowledge we are indebted to the late Dr. Robert Smith of Cambridge, a very eminent geometer and philosopher, and a good judge of music, and very pleasing performer on the organ and harpsichord. This gentleman, in his Dissertation on the Principles of Harmonics, published for the first time in 1749, has paid particular attention to a phenomenon in co-existent sounds, called a *beating*. This is an alternate enforcement and diminution of the strength of sound, something like what is called a close shake, but differing from it in having no variation in the pitch of the sounds. It is a sort of undulation of the sound, in which it becomes alternately louder and fainter. It may be often perceived in the sound of bells and musical glasses, and also in the sounds of particular strings. It is produced in this way: Suppose two unisons quite perfect; the vibrations of each are either perfectly co-incident, or each pulse of one sound is interposed in the same situation between each pulse of the other. In either case they succeed each other with such rapidity, that we cannot perceive them, and the whole appears an uniform sound. But suppose that one of the sounds has 240 pulses in a second, which is the undulation that is produced in a pipe of 24 inches long; suppose that the other pipe is only 23 inches and $\frac{1}{240}$ths long. It will give 243 pulses in a second. Therefore the 1st, the 80th, the 160th, and the 240th pulse of the first pipe will coincide with the 1st, the 81st, the 162d, and the 243d pulse of the other. In the instants of coincidence, the agitation produced by one pulse is increased by that produced by the other. The commencement of the next two pulses is separated a little, and that of the next is separated still more, and so on continually: the *dislocations* of the pulses, or their deviations from perfect coincidence, continually increasing, till we come to the 40th pulse of the one pipe, which will commence in the middle of the 41st pulse of the

other pipe; and the pulses will now bisect each other, so that the agitations of the one will counteract or weaken those of the other. Thus the compounded sound will be stronger at the coincidences of the pulses, and fainter when they bisect each other. This reinforcement of sound will therefore recur thrice in every second. The frequency of the pulses are in the ratio of a comma, or 81 : 80. Therefore this constitutes an *unison imperfect by a comma.* If therefore any circumstance should require that these two pulses should form an unison imperfect by a comma, we have only to alter one of the pipes, till the two, when sounded together, beat thrice in a second. Nothing can be plainer than this. Now let us suppose a third pipe tuned an exact fifth to the first of these two. There will be no beating observable; because the recurrence of coincident pulses is so rapid as to appear a continued sound. They recur at every second vibration of the bass, or 120 times in a second. But now, instead of sounding the third pipe along with the first, let it sound along with the second. Dr. Smith demonstrates, that they will beat in the same manner as the unisons did, but thrice as often, or nine times in a second. When therefore the fifth C *g* beats nine times in a second, we know that it is too sharp or too flat (very nearly) by a comma.

368. Dr. Smith shews, in like manner, what number of beats are made in any given time by any concord, imperfect or tempered, in any assigned degree. We humbly think that the most inattentive person must be sensible of the very great value of this discovery. We are obliged to call it *his* discovery. Mersennus, indeed, had taken particular notice of this undulation of imperfect consonances, and had offered conjectures as to their cause; conjectures not unworthy of his great ingenuity. Mr. Sauveur also takes a still more particular notice of this phenomenon *, and makes a most ingenious use of it for the solution of a very important musi-

* Mem. Acad. Par. 1701. 1702. 1707. and 1713.

cal problem: namely, to determine the precise number of pulses which produce any given note of the gamut. His method is indeed operose and delicate, even as simplified and improved by Dr. Smith. The following may be substituted for it, founded on the mechanism of sounding chords. Let a violin, guitar, or any such instrument, be fixed up against a wall, with the finger-board downward, and in such a manner, that a violin string strained by a weight, may press on the bridge, but hang free of the lower end of the finger-board. Let another string be strained by one of the tuning pins till it be in unison with some note (suppose C) of the harpsichord Then hang weights on the other strings, till, upon drawing the bow across both strings, at a small distance below the bridge, they are perfect unisons, without the smallest beating or undulation, and taking care that the pressure of the bow on that string which is tuned by the pin be so moderate as not to affect its tension sensibly. Note exactly the weight that is now appended to it. Now increase this weight in the proportion of the square of 80 to the square of 81; that is, add to it its 40th part very nearly. Now draw the bow again across the strings with the same caution as before. The sounds will now beat remarkably; for the vibrations of the loaded string are now accelerated in the proportion of 80 to 81. Count the number of undulations made in some small number (suppose 10) of seconds. This will give the number of beats in a second; 80 times this number are the single pulses of the lowest sound; and 81 times the same number gives the pulses of the highest of these imperfect unisons.

If this experiment be tried for the C in the middle of our harpsichords, it will be found to contain 240 pulses very nearly: for the strings will beat thrice in a second. The beats are best counted by means of a little ball hung to a thread, and made to keep time with the beats.

369. Here, then, is a phenomenon of the most easy observation, and requiring no skill in music, by which the pitch

of any sound, and the imperfection of any concord, may be discovered with the utmost precision; and by this method may concordant sounds be produced, which are absolutely perfect in their harmony, or having any degree of imperfection or temperament that we please. An instrument may generally be tuned to perfect harmony, in some of its notes, without any difficulty, as we see done by every blind Crouder. But if a certain determinate degree of imperfection, different perhaps in the different concords, be necessary for the proper performance of musical compositions on instruments of fixed sounds, such as those of the organ or harpsichord kind, we do not see how it can be disputed that Dr. Smith's theory of the beating of imperfect consonances is one of the most important discoveries, both for the practice and the science of music, that have been offered to the public. We are inclined to consider it as the most important that has been made since the days of Galileo. The only rivals are Dr. Brook Taylor's mechanical demonstration of the vibrations of an elastic cord, and its companion, and of the undulations of the air in an organ pipe, and the beautiful investigations of Daniel Bernoulli of the harmonic sounds which frequently accompany the fundamental note. The musical theory of Rameau we consider as a mere whim, not founded in any natural law; and the theory of the grave harmonics by Tartini or Romieu is included in Dr. Smith's theory of the beating of imperfect consonances. This theory enables us to execute any harmonic system of temperament with precision, and certainty, and ease, and to decide on its merit when done.

We are therefore surprised to see this work of Dr. Smith greatly undervalued, by a most ingenious gentleman in the Philosophical Transactions for 1800, and called a large and obscure volume, which leaves the matter just as it was, and its results useless and impracticable. We are sorry to see this; because we have great expectations from the future labours of this gentleman in the field of harmonics, and his

late work is rich in refined and valuable matter. We presume humbly to recommend to him attention to his own admonitions to a very young and ingenious gentleman, who, he thinks, proceeded too far in animadverting on the writings of Newton, Barrow, and other eminent mathematicians. We also beg his leave to observe, that Dr. Smith's application of his theory may be very erroneous (we do not say that it is perfect), in consequence of his notion of the proportional effects produced on the general harmony by equal temperaments of the different concords. But the theory is untouched by this improper use, and stands as firmly as any proposition in Euclid's Elements. We are bound to add to these remarks, that we have oftener than once heard music performed on the harpsichord described in the second edition of Dr. Smith's Harmonics, both before it was sent home by the maker (the first in his profession) and afterwards by the author himself, who was a very pleasing performer, and we thought its harmony the finest we ever heard. Mr. Watt, the celebrated engineer, and not less eminent philosopher, built a handsome organ for a public society, and, without the least ear or relish for music, tuned three octaves of the open diapason by one of Dr. Smith's tables of beats, with the help of a variable pendulum. Signior Doria, leader of the Edinburgh concert, tried it in presence of the writer of this article, and said, "Bellissima—sopra modo bellissima!" Signior Doria attempted to sing along with it, but would not continue, declaring it impossible, because the organ was ill-tuned. The truth was, that, on the major key of $E^{\flat}$, the tuning was exceedingly different from what she was accustomed to, and she would not try another key. We mention this particular, to shew how accurately Mr. Watt had been able to execute the temperament he intended.

370. This theory is valuable, therefore, by giving us the management of a phenomenon intimately connected with harmony, and affording us precise and practicable measures of all deviations from it. It bids fair, for this reason, to

give us a method of executing any system of temperament which we may find reason to prefer. But we have another ground of estimation of this theory. By its assistance, we are able to ascertain with certainty and precision the true untempered scale of music, which eluded all the attempts of the ingenious Greeks; and we determine it in a way suited to the favourite music of modern times, of which almost all the excellencies and pleasures are derived from harmony. We do not say that this *total* innovation in the principle of musical pleasure is unexceptionable; we rather think it very defective, believing that the thrilling pleasures of music depend more upon the melody or air. We appeal even to instructed musicians, whether the heart and affections are not more affected *(and with much more distinct variety of emotion)* by a fine melody, supported, but not observed, by harmonies judiciously chosen? It appears to us that the effect of harmony, always filled up, is more uniformly the same, and less touching to the soul, than some simple air sung or played by a performer of sensibility and powers of utterance. We do not wonder, then, that the ingenious Greeks deduced all their rules from this department of music, nor at their being so satisfied with the pleasures which it yielded, that they were not solicitous of the additional support of harmony. We see that melody has suffered by the change in every country. There is no Scotchman, Irishman, Pole, or Russian, who does not lament that the skill in composing heart-touching airs is degenerated in his respective nation; and all admire the productions of their muse of "the days that are past." They are "pleasant and mournful to the soul."

But we still prefer the harmonical method of forming the scale, on account of its precision and facility: and we prefer the theory of beats, *because it also gives us the most satisfactory scale of melody;* and this, not by repeated corrections and recorrections, but by a direct process. By a table of beats, every note may be fixed at once, and we have no occasion to return to it and try new combinations; for the beatings of the different concords to one bass being once de-

termined, every beating of any one note with any other is also fixed.

371. We therefore request the reader's patient attention to the experiment which we have now to propose. This experiment is best made with two organ pipes equally voiced, and pitched to the note C in the middle of our harpsichords. Let one of them at least be a stopped pipe, its piston being made extremely accurate, and at the same time easily moved along the pipe. Let the shank of it be divided into 240 equal parts. The advantage of this form of the experiment is, that the sounds can be continued, with perfect uniformity, for any length of time, if the bellows be properly constructed. In default of this apparatus, the experiment may be made with two harpsichord wires in perfect unison, and touched by a wheel rubbed with rosin instead of a bow, in the way the sounds of the vielle or hurdygurdy are produced. This contrivance also will continue the sounds uniformly at pleasure. A scale of 240 parts must be adapted to one string, and numbered from that end of the string where the wheel or bow is applied to it. Great care must be taken that the shifting of the moveable bridge do not alter the strain on the wire. We may even do pretty well with a bow in place of the wheel; but the sound cannot be long held on in any pitch. In describing the phenomena, we shall rather abide by the string, because the numbers of the scale, or length of the sounding part of the wire, correspond, in fact, much more exactly with the sounds. The deviations of the scale of the pipe do not in the least affect the conclusions we mean to draw, but would require to be mentioned in every instance, which would greatly complicate the process.

Having brought the two open strings into perfect unison, so that no beating whatever is observed in the consonance, slide the moveable bridge slowly along the string while the wheel is turning, beginning the motion from the end most remote from the bow. All the notes of the octave,

and all kinds of concords and discords, will be heard; each of the concords being preceded and followed by a ruffling beating, and that succeeded by a grating discord. After this general view of the whole, let the particular harmonious stations of the bridge be more carefully examined as follows.

372.—I. Shift the moveable bridge to the division 120. If it has been exactly placed, we shall hear a perfect octave without any beating. It is, however, seldom so exactly set, and we generally hear some beating. By gently shifting the bridge to either side, this beating becomes more or less rapid; and when we have found in which direction the bridge must be moved, we can then slide it along till the beating cease entirely, and the sounds coalesce into one sound. We can scarcely hear the treble or octave note as distinguishable from the bass or fundamental afforded by the other string. If the notes are duly proportioned in loudness, we cannot hear the two as distinct sounds, but a note seemingly the same with the fundamental, only more brilliant. (*N. B.* It would be a great improvement of the apparatus to have a micrometer screw for producing those small motions of the bridge.)

Having thus produced a fine octave, we can now perceive that, as we continue to shift the bridge from its proper place, in either direction, the beating becomes more and more rapid, changes to a violent rattling flutter, and then degenerates into a most disagreeable jar. This phenomenon is observed in the deviation of every concord whatever from perfect harmony, and must be carefully kept in remembrance.

373. Before we quit this concord, the octave produced by the bisection of the pipe or string, we must observe, that with respect to ourselves, the octave $c\ \bar{c}$ must beat almost twice in a second, before we can observe clearly any mis-tune in it, by sounding the notes in succession, or as steps in the scale of melody. We never knew any ear so nice as to discover a mis-tuning when it beats but once in three seconds.

We think ourselves entitled therefore to say, that we are insensible of a temperament in melody amounting to one-third of a comma; and we never knew a person sensible of a temperament half this bulk.

When the imperfection of the octave is clearly sensible by sounding the notes in succession, it is extremely disagreeable, feeling like a struggle or endeavour to attain a certain note, and a failure in the attempt. This seems owing to the familiar similarity of octaves, in the habitual talking and singing of men and women together. But when the notes are sounded together, although we are not much more sensible of the imperfection of the harmony directly, as a failure in the sweetness of the concord, we are very sensible of this phenomenon of beating; and any person who can distinguish a weak sound from a stronger one, can easily perceive, in this indirect manner, any fraction of a comma, however minute. This makes the tuning by harmony much more exact than by melody alone. It is also much more accommodated to the genius of modern music. The ancients had favourite passages, which were frequently introduced into their airs, and they were solicitous to have these in good tune. It appears from passages in the writings of Galen, that different performers excelled chiefly in their skill in making those occasional temperaments which their music required. Our music is much more strict, by reason of our harmonic accompaniments, which are an abominable noise when mis-tuned in a degree, which would have passed with the ancients for very good melody. Aristoxenus says, that the ear cannot discover the error of a comma. This would now be intolerable.

374. But another advantage attends our method. We obtain, by its assistance, the most perfect scale of melody perfect in a degree attainable only by chance by the Greeks. This is now to be our business to unfold.

375.—II. Set the moveable bridge at 158, and sound the two strings. They will beat very disagreeably, being

plainly out of tune. Slide it gradually toward 160, and the beats will grow slower and slower; will change to a gentle and not unpleasant undulation; and at last, when the bridge is at 160, will vanish entirely, and the two sounds will coalesce into one sweet concord, in which neither of the component sounds can be distinguished. If the sound given by the short string be now examined as a step in the scale of melody, it will be found a fifth to the sound of the long string or fundamental note, perfectly satisfactory to the nicest ear. Thus one step of the scale has been ascertained.

III. Slide the bridge slowly along the string. The beating will recommence, will become a flutter, and then a jarring noise; and will again change to an angry flutter, beating about eight times in a second, when the bridge stands at 169 nearly. Pushing it still on, but very slowly, the flutter will become an indistinct jarring noise; which, by continuing the motion, will again become a flutter, or beat about six in the second. The bridge is now about 171.

376.—IV. Still continuing the motion, the flutter becomes a jarring noise, which continues till the bridge is near to 180, when the rapid flutter will again be heard. This will become slower and slower as we approach to 180; and when the bridge reaches that point, all beating vanishes, and we have a soft and agreeable concord, but far inferior to the former concord in that cheering sweetness which characterises the fifth. When this note is compared with that of the fundamental string, as a step in the scale of melody, it is found to correspond to the note *fa*, or the fourth step in the scale, and in that employment to give complete satisfaction to the ear.

377.—V. Still advancing the moveable bridge toward the nut, we shall hear the beatings return again; and after fluttering and degenerating to a jarring noise, by a very small motion of the bridge, they will again be heard, will grow slower, accompanied with a sort of angry expression, and will cease entirely when the bridge reaches the 192d division of

our scale. Here we have another concord of very peculiar character, being remarkably enlivening and gay. This sound gives perfect satisfaction to the ear, if employed as the third step in the scale of melody, being the note *mi* of that series, at least in all gay or cheerful airs.

378.—VI. As we move the bridge from 192 to 200, we hear again the same beatings, which, in the immediate vicinity to 192, have a peevish, fretful expression, instead of the angry waspish expression before mentioned. When the bridge has passed that situation which produces only grating discordance, we hear the beatings again, and they become slower, and cease altogether when the bridge arrives at 200. Here we have another consonance, which must be called a *concord*, because it is rather agreeable than otherwise, but strongly marked by a mournful melancholy in the expression. In the scale of melody, it forms the third step in those airs which express lamentation or grief. It is called the *minor third*, to distinguish it from the last enlivening concord, which, being a larger interval, is called the *major third*.

379. It is well known, that these two thirds give the distinguishing characters to the only two modes of melodious composition that are admitted into modern music. The series containing the major third is called the *major*, and that containing the minor third is called the *minor mode*. It is worthy of remark, that the fanatical preachers, in their conventicles and field sermons, affect this mode in their harangues, which are often distinctly musical, modulating entirely by musical intervals, and keeping the whole of their chaunt in subordination to a fundamental or key note. This is not unnatural, when we consider the general scope of their discourses, namely to inspire melancholy and humiliating thoughts, awakening sorrow, and the like. It is not so easy to account for the usual whine of a beggar, who generally craves charity in the major third. This is the case, at least, in the northern parts of this island.

380. If we continue to shift the bridge still nearer to the end of the string, we shall hear nothing but a succession of

vile discordant noises, somewhat less offensive when the bridge is about the divisions 213 and 216, but even there very unpleasant.

381.—VII. Let us therefore change our manner of proceeding a little, and again place the bridge at 160, which will give us the pleasing concord of the fifth. Instead of pushing it from that place toward the nut, let it be moved toward the wheel or bow. Without repeating what we have said of the appearance of the beatings, their acceleration, and their degenerating into a jarring discord, to be afterwards succeeded by another beating, &c. &c. we shall only observe, that when we place the bridge at 150, we have no beatings, and we hear a consonance, which is in a slight degree pleasant, and may therefore be called a *concord*. It has the other marks of a concord which we have been making so much use of; for the beatings recommence when we shift the bridge to either side of 150. This note makes the sixth step in the descending scale of mournful melody: that is, when we are passing from the acute to the graver notes, with the intention of putting an emphasis on the third and the fundamental. Although not eminent as a concord with the fundamental alone, it has a most pleasing effect when listened to in subordination to the whole series, or when sounded along with other proper accompaniments of the fundamental.

382.—VIII. Placing the bridge at 144, we obtain another very pleasing concord, differing in its expression from any of the foregoing. We find it difficult to express its character. It is greatly inferior to the fifth in sweetness, and to the major third in gaiety, but seems to possess, in a lower degree, both of these qualities. In the scale of cheerful melody, it is the sixth note, which we have distinguished by the syllable *la*. It is also used even in mournful melody, when we are ascending, with the intention of closing with the octave.

383. In shifting the bridge from 144 to 120, we obtain nothing but discordant, or at least disagreeable consonances.

And, lastly, if we move the bridge beyond 120, to divisions which are respectively the halves of those numbers which produced the concords already treated of, we obtain the same steps in the scale of the upper octave. Thus if the bridge be at 80, we have the fifth to the octave note, or twelfth to the fundamental. If it be at 60, we obtain the double octave, &c &c. &c.

384. We have perhaps been rash in affixing certain moral or sentimental characters to certain concords; for we have seen instances of persons who gave them different denominations; but these were never contradictory to ours, but always expressed some sentiment allied to that which we have assigned. We never met with an instance of a person capable of a little discriminating reflection, who did not acknowledge a manifest sentimental distinction among the different concords which could not be confounded. We doubt not but that the Greeks, a people of exquisite sensibility to all the beauties of taste and sentiment, paid much attention to these characters, and availed themselves of them in their compositions. We do not think it at all unlikely, that greater effects have been produced by their music, which was studied with this express view, than have ever been produced by the modern music, with all the addition of harmony. We have allowed too great a share of our attention to mere harmony. Our great authors are much less solicitous to compose an enchanting air, than to construct a full score of rich and well conducted harmony. We do not profess to be nice judges in musical composition, but we may tell what we ourselves experience. We find our minds worked up by a continuance of fine harmony into a *general* sensibility; into a frame of mind which would prepare and fit us for receiving strong impressions of moral sentiment, if these were distinctly made. But we have seldom felt any distinct emotions excited by mere instrumental music. And when the harmonies have been merely to support the performance of a voice, the words have been either so frittered by musical di-

visions, as to become in some measure ludicrous—or have been so indistinct, and made so trifling a part of the music, that there was nothing done to give a particular shape to the moral impression on our mind. We have generally been strongly affected by some of the anthems which were in vogue in former times; and we think that we perceived the cause of this difference: There was a great simplicity in the voice parts: the syllables were not drawled out into long musical phrases, but pronounced nearly according to their proper quantities; so that the sentiment of the speaker was expressed with all the force of good declamation, and the harmony of the accompaniment then strengthened the appropriate effect of the melody. We mean not to offer these observations as of much authority, but merely to mention some facts, and to assign what we felt to be their causes, in order to promote, in some degree, however insignificant, the cultivation of musical science. With this view, we venture to say, that some of the best compositions of Knapp of York *uniformly* affect us more than the more admired anthems of Bird and Tallis. A cadence, which Knapp gives almost entirely to the melody, is laboured by Bird or Tallis with all the rules of art; and you have its characters of perfect or imperfect, full or disappointed, cadences, and such an apparatus of preparation and resolution of discords, that you forsee it at the distance of several bars, and then the part assigned to the voice seems a very trifle, and merely to fill up a blank in the harmony. Such compositions smell of the lamp, and fail of their purpose, that of charming the *learned* ear. But enough of this digression.

385. Thus have we found a natural relation between certain sounds strongly marked by very precise characters. The concordance of sound is marked by the absence of all undulation, and the deviations from this harmony are shewn to be measurable by the frequency of those undulations. We have also found, that the notes which are thus harmonious along with the fundamental, are steps in the scale of natural

music (for we must acknowledge melody to be the primitive music dictated by nature.) We have got the notes—*do*—*mi*, *fa*, *sol*, *la*—*do*, ascertained in a way that can no longer be mistaken.

386. Let us now examine what physical or mechanical relations these sounds stand in to each other. Our monochord gives us the lengths of the strings; and the discovery of Galileo shews us, that these are also the durations of the aerial pulses which produce the sensations of musical notes. Their ratios may therefore be truly called the ratios of the sounds. Now we see that the strings which produce the sounds *do sol* are 240 and 160. These are in the ratio of 3 to 2. In this manner we may state all the ratios observed in our experiment, *viz.*

Do : *mi* have the ratio of	240 to 192,	or of 5 to 4
Do : *fa*	240 : 180	4 : 3
Do : *sol*	240 : 160	3 : 2
Do : *la*	240 : 144	5 : 3
Mi : *sol*	192 : 160	6 : 5, = *do* : *mi*
Fa : *sol*	180 : 160	9 : 8
Sol : *la*	160 : 144	10 : 9
Mi : *fa*	192 : 180	16 : 15

Here we get the sight of all the ratios which the ingenious and unwearied speculations of the Greek mathematicians enlisted into the service of music, without being able to give a good reason why. The ratio 5 : 4, which their fastidious metaphysicians rejected, and which others wished to introduce from motives of mere necessity to fill up a blank, is pointed out to us by one of the finest concords. The interval between the fourth and fifth is, *very fortunately*, a step of the scale.

387. The next step *sol la* is more important. For the ear for melody would have been very well satisfied with an interval equal to *fa sol*, or 9 : 8; but if the moveable bridge be set at the division $142\frac{2}{9}$, corresponding to such a step, we should have a very offensive fluttering. It is reasonable

therefore to conclude, *from analogy*, that the interval *sol la* does not correspond to the ratio 9 : 8 ; and that 10 : 9, which is, at least, equally satisfactory to the ear, is the proper step, even in the scale of melody. If we consider what may be called the scale of harmony, there is no room left for doubt. To enjoy the greatest possible pleasure of harmony, we must not only take each note as it is related to the fundamental, but also as it is related to other notes of the scale. It may chance to be convenient to assume for the fundamental of our occasional scale of modulation, the string of the lyre which is tuned as *fa* to its proper fundamental ; or it may increase the harmony (and we know that it does), if we accompany the note *do* with both of the notes *fa* and *la*. To have the fine concord of the major third, it is necessary that the interval *fa la* be equivalent to the ratio 5 : 4. Now *fa* is 180, and 5 : 4 = 180 : 144. Therefore, by making the step *sol la* equal to 9 : 8, we should lose this agreeable concord, and get discord in its place.

And thus is evinced, in opposition to Aristoxenus, the propriety of having both a major and a minor tone ; the first expressed by 9 : 8, and the last by 10 : 9. The difference between these steps is the ratio 81 : 80, called a comma by the Greek theorists.

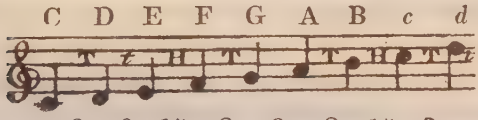
388. We still want two steps of the scale, and two sounds or notes corresponding to them, namely *re* and *si ;* and we wish to establish them on the same authority with the rest. We see that this cannot be done by a concordance with the fundamental *do*. The ear sufficiently informs us that the steps *do re* and *la si* must be tones, and not semitones, like *mi fa*. The sensible similarity of the two tetrachords, *do re mi fa* and *sol la si do*, also teaches us that the step *si do* should be a semitone like *mi fa*. This seems to be all that mere melody can teach us. But we have little information whether we shall make *la si* a major or a minor tone. If we copy the tetrachord *do re mi fa* exactly, we shall make the step *si do* like *mi fa*, and equivalent to the ratio 16 : 15.

This requires the moveable bridge to be placed at 128. The sound produced by this division is perfectly satisfactory to the ear as a step of the scale of melody. Moreover, our satisfaction is not confined to the comparison of it with the note *do*, into which we slide by this gentle step. It makes agreeable melody when used as the third to the note *sol*. If we examine it mathematically, we find it a perfect major third to *sol*; for *sol* requires the 160th division. Now 160 : 128 = 5 : 4, which is the ratio of the pulses of a major third. All these reasons seem enough to make us adopt this determination of the note *si*.

389. It remains to consider how we shall divide the interval *do—mi*. It is a perfect major third. So is *fa la*, and so is *sol si*. But in the first of these two, we have seen that it must be composed of a major tone with a minor tone above it; and in the second we have a minor tone followed by a major tone above. We are left uncertain therefore whether *do re* shall resemble *fa la* or *sol si* in the position of its two parts. Aristoxenus and his followers declared the ear to be equally pleased with both. Ptolemy's *Systema Diatonicum Intensum* makes *do re* a major tone, and other systems make it minor. Even in modern times it has been considered as uncertain; and the only reason which we have to offer for a preference of the major tone for the first step is, that, so far as we can judge by our own feelings, the sounds in the relation of 9 : 8 are less discordant than sounds in the relation of 10 : 9, and because all the other steps have been determined by means of concords with the key. We refer, for a more particular examination of the principles on which these arrangements are valued, to *Dr. Smith's Harmonics*, Prop. I. where he shews how one is preferable to another, in proportion as it affords a greater number of perfect concords among the neighbouring notes, which is the favourite object in all modern music. Upon this principle our arrangement is by far the best, because it admits five more concords in the octave than the other. But we have con-

sidered the subject in a different manner merely to avail ourselves of the phenomenon by which all the steps, except one, seem to be naturally ascertained, and by which the connection between harmony and melody seems to be pointed out to us.

390. It will be convenient to represent the tones major and minor and the hemitone, by the symbols T, *t*, and H. Also to mark the notes by the Roman numerals, or by cyphers according as they are the extremes of major or minor intervals. By this notation the octave may be represented thus:



$$\frac{8}{9}\ \frac{9}{10}\ \frac{15}{16}\ \frac{8}{9}\ \frac{9}{10}\ \frac{8}{9}\ \frac{15}{16}\ \frac{8}{9}\ \&c.$$

K II III 4 V VI VII VIII IX X &c.

The reader will remark, that the primary divisions which we assigned to the representation of an octave in Plate V. fig. 1. by the circumference of a circle, are in conformity to this Ptolemaic partition of the octave. He will also be sensible, that the division into five equal mean tones and two equal hemitones, which is expressed by the dotted lines, agreeing with the Ptolemaic division only at C and E, is effected by bisecting the arch CE; and therefore the deviation of the sound substituted for the Ptolemaic D is half the difference of CD and DE, that is, half a comma. The deviations therefore at F, G, A, and B, are each a quarter of a comma.

It is well known, that if the logarithm of the length of one string be subtracted from that of another, the difference is a measure of the ratio between them. Therefore 30103 is the measure of the musical interval called the octave, and then the measures of the

Comma . .	540 or	54
Hemitone . .	2803	280

Minor tone . .	4576 or	458
Major tone . .	5115	512
3d . . .	7918	792
IIId . .	9691	969
4th . . .	12494	1249
Vth . . .	17609	1761
6th . . .	20412	2041
VIth . .	22185	2219
VIIth . . .	27300	2730
VIIIth . .	30103	3010

This is a very convenient circumstance. If we take only the four first figures as integers, and make the octave consist of 3010 parts, we have a scale more exact than the nicest harmony requires. The circumference of a circle may be so divided into 301 degrees, and the moveable circle have a nonius subdividing each into 10. Or it may be divided into 55,8 degrees, each of which will be a comma. Either of these divisions will make it a most convenient instrument for expeditiously examining all temperaments of the scale that can be proposed. Or a straight line may be so divided, and repeated thrice. Then a sliding ruler, divided in the same manner, and applied to it, will answer the same purpose. We shall see many useful employments of these instruments by and by.

391. Having thus endeavoured to communicate some plain notion of the formation and singular nature of that gradation of sounds which produces all the pleasures of music, and of the manner of obtaining the steps of this gradation with certainty and precision, we proceed to consider how those musical passages may be performed on such keyed instruments as the organs and harpsichords, as they are now constructed. These instruments have twelve sounds and intervals in every octave, in order that an air may be performed in any pitch; that is, taking any one of the sounds as a key note. It is plain that this cannot be done with accuracy; for we have now seen that the interval *mi fa* is bigger

than half of *do re* or *re mi*, &c. and therefore the intercalary sound formerly mentioned to be inserted between C and D, D and E, &c. will not do indiscriminately for the sharp of the sound below, and the flat of the sound above it. When the tones are reduced to a mean size, the ear is scarcely sensible of the change in melody, and the harmony of the fifths and fourths is not greatly hurt. But when the half notes are inserted, and employed to make up harmonious intervals, as recommended by Zarlino, the harmony is very coarse indeed.

392. But we must make the reader sensible of the necessity of some temperament, even independent of those artificial notes. Therefore

Let the scholar tune upwards the four Vths *c g*, *g* $\overline{d}$, $\overline{d}$ $\overline{a}$, $\overline{\overline{a}}$ $\overline{\overline{e}}$, all perfect, admitting no beating whatever. This is easily done, either with the organ or the wheel monochord already described. Then tune downwards the perfect octaves $\overline{\overline{e}}$ *e*, $\overline{e}$ *e*. Now examine the IIId *c e* which results from this process. If the instrument be of the pitch hitherto supposed (*c* making 240 pulses in a second,) this IIId will be heard beating 15 times in a second, which is a discordance altogether intolerable, the note *e* being too sharp in the ratio of 81 to 80, which makes a comma. It is easily found, by calculation, that *e* makes $303\frac{3}{4}$ pulses, instead of 300, required for the IIId to *c*.

N. B. It may not be amiss to inform our readers, that if any concord, whose perfect ratio is $\frac{m}{n}$ (*m* being the greatest term of the smallest integers expressing that ratio) be tempered sharp by the fraction $\frac{p}{q}$ of a comma, and if M and N be the pulses made by the acute and grave notes of the concord during any number of seconds, the number *b* of beats made in the same time by this concord will be $= \frac{2\,q\,m\,\mathrm{N}}{161\,p - q}$.

or $\frac{2\,q\,n\,M}{161\,p+q}$; and if it be tempered flat, then $b=\frac{2\,q\,m\,N}{161\,p+q}$, or $\frac{2\,q\,n\,M}{161\,p-q}$. (*Smith's Harm.* 2d edit. p. 82, &c.)

393. It is impossible, therefore, to have perfect Vths and perfect IIIds at the same time. And it will be found, that the 3d *e g* resulting from this process, and the VIth *c a*, are still more discordant, rattling at an intolerable rate. Now the major and minor thirds, alternately succeeding each other, form the greatest part of our harmonies; and the VIth is also a very frequent accompaniment. It is necessary therefore to sacrifice somewhat of the perfect harmony of the Vths, in order that we may not be disgusted with the discord of those other harmonies: and it is this mutual accommodation, and not the changes made necessary by the introduction of intercalary notes, which is properly called TEMPERAMENT. It will greatly assist us in understanding the effects of the temperaments of the different concords, if we examine all the divisions of the circular representation of the octave and musical scale given in Plate V. fig. 1. by placing the index of the moveable circle on that note of the outer circle for which we want the proper harmonies, or accompaniments, which are either the IIId and Vth, or the 4th and VIth. We shall thus learn, in the *first* place, the deviations of the different perfect notes of the scale from the notes required for this new fundamental; and we must then study what effect the same temperament produces on the agreeableness of the harmony of different concords having the same bass or the same treble, taking it for granted that the hurt to the harmony of any individual concord is proportional to its temperament.

394 It is in this delicate department of musical science that we think the great merit of Dr. Smith's work consists. We see that the deviation from perfect harmony is always accompanied with beats, and increases when they increase

in frequency—whether it increases in the same proportion may be a question. We think that Dr. Smith's determination of the equality of imperfect harmony in his 13th proposition, includes every mathematical or physical circumstance that appears to have any concern in it. What relates immediately to our sensations is, as yet, an impenetrable secret. The theory of beats, as delivered by this author, affords very easy, though sometimes tedious, methods of measuring and of insuring all the varieties which can obtain in the beating of imperfect consonances. It appears to us therefore very unjust to say, with a late writer in the Philosophical Transactions*, that this obscure volume has left the matter where it found it. The author has given us *effective* principles, although he may have been mistaken in the application; which however we are far from affirming. Our limits will not allow us to give any account of that theory; and indeed our chief aim in the present article is to give a method of temperament which requires no scientific knowledge of the subject. But we could not think of losing the opportunity of communicating, by the way, to unlearned persons, some more distinct notions of the scale of musical sounds, and of its foundation in nature, than scholars usually receive from the greater number of mere music masters. The acknowledged connection of the musical ratios with the pleasures of harmony and melody, has (we hope) been employed in an easy and not obscure manner; and the phenomena which we have faithfully narrated, shew plainly that, by diminishing the rattling undulations of tempered concords, we are certain of improving the harmony of our instruments. We shall proceed therefore on this principle for the use of the mere performer, but at the same time introducing some very simple deductions from Smith's theory, for which we

* Dr. Thomas Young, to whom Dr. Robison here alludes, has published a reply to this, and a preceding part of the present article, in Nicholson's Journal, for August 1801. See also Dr. Young's *Lectures on Natural Philosophy*, vol. ii. p. 607.

expect the thanks of all such readers as wish to see a little of the reasons on which they are to proceed.

395. The experiment, of which we have just now given an account, shews that four consecutive fifths compose a greater interval than two octaves and a major third. Yet, in the construction of our musical instruments of fixed sounds, they must be considered as of equal extent; since we have 7 half intervals in the Vth, and 12 in the octave, and four in the IIId, four Vths contain 28, and two octaves contain 24; and these, with the four which compose a IIId, make also 28. It is plain, therefore, that whatever we do with the IIIds, we must lessen the Vths. If therefore we keep the IIId perfect, we must lessen each of the Vths by $\frac{1}{4}$th of a comma; for we learned, by the beating of the imperfect IIId *c e*, that the whole excess of the four Vths was a comma. Therefore the Vth *c g* must be flattened $\frac{1}{4}$th of a comma. But how is this to be done with accuracy? Recollect the formula given a little ago, where the number of beats b in any number of seconds is $= \frac{2\,q\,m\,\mathrm{N}}{161\,p\,+\,q}$. In the present case $q = 1$, $m = 3$, $\mathrm{N} = 240$ per second, and $p = 4$. Therefore the formula is $= \frac{2 \times 3 \times 240}{161 \times 4 + 1} = \frac{1440}{645} = 2{,}25$ in a second, or 9 beats in four seconds very nearly.

In like manner, the next Vth *g* $\overline{d}$ must be flattened $\frac{1}{4}$th of a comma, by making it beat half as fast again, or $13\frac{1}{2}$ beats in four seconds (because in this Vth $\mathrm{N} = 360$. But as this beating is rather too quick to be easily counted, it will be better to tune downwards the perfect octave *g* G, which will reduce N to 180 for the Vth G *d*. This will give us 1,68 per second, or 10 beats in 6 seconds very nearly.

There is another way of avoiding the employment of too quick beats. Instead of tuning the octave *g* G, make *c* G beat as often as *c g*. This is even more exactly an octave to *g* than can be estimated by a good ear. Dr. Smith has

demonstrated, that when a note makes a minor concord with another note below it, and therefore a major concord with the octave to that note, it beats equally with both; but if the major concord be below, it beats twice as fast with the octave above. Now in the present case, *c g* is a Vth, and *c* G a 4th. For the same reason *c f* would beat twice as fast as *c* F.

In the next place, the Vth *d* $\bar{a}$ must be made to beat flat 15 times in 6 seconds.

In like manner, instead of tuning upward the Vth $\bar{a}$ $\bar{\bar{e}}$, tune downward the octave *a* $\bar{a}$, and then tune upward the Vth *a e*, and flatten it till it beat 15 times in 8 seconds.

If we take 15 seconds for the common period of all these beats, we shall have

The beats of *c g* = 34.
G *d* = 25.
d $\bar{a}$ = 37½.
a c = 28.

396. We shall now find *c e* to be a fine IIId, without any sensible beating; and then we proceed in the same way, always tuning upward a perfect Vth; and when this would lead too high, and therefore produce too quick beating, we should tune downward an octave. Do this till we reach *b* ♯, which should be the same with $\bar{c}$, or a perfect octave above *c*. This will be a full proof of our accurate performance. But the best process of tuning is to stop when we get to *g c*. Then we tune Vths downward from *c*, and octaves upward when the Vths would lead us too low. Thus we get *c* F, F *f*, *f* $b^{\flat}$, $b^{\flat}$ $\overline{b^{\flat}}$, $\overline{b^{\flat}}$ $c^{\flat}$, and thus complete the tuning of an octave. We take this method, instead of proceeding upwards to $\bar{b}$ ♯; because those notes marked sharp or flat are, when tuned in this way, in the best relation to those with which they are most frequently used as IIIds.

397. This process of temperament will be greatly expedited by employing a little pendulum, made of a ball of

about two ounces weight, sliding on a light deal rod, having at one end a pin hole through it. To prepare this rod, hang it upon a pin stuck into the wainscoating, and slide the ball downward, till it makes 20 vibrations in 15″, by comparing it with a house clock. In this condition mark the rod at the upper edge of the ball. In like manner, adjust it for 24, 28, 32, 36, 40, 44, 48, vibrations, making marks for each, and dividing the spaces between them by the eye, noticing their gradual diminution. Then, having calculated the beats of the different Vths, set the ball at the mark suited to the particular concord, and temper the sound till the beats keep pace exactly with the pendulum.

398. But previous to all this, we must know the number of pulses made in a second by the C of our instrument. For this purpose we must learn the pulses of our tuning fork. To learn this, a harpsichord wire must be stretched by a weight till it be unison or octave below our fork: then, by adding $\frac{1}{40}$th of the weight to what is now appended, it will be tempered by a comma, and will beat, when it is sounded along with the fork; and we must multiply the beats by 80: The product is the number of pulses required. And hence we calculate the pulses of the C of our instrument when it is tuned in perfect concord with the fork.

The usual concert pitch and the tuning forks are so nearly consonant to 240 pulses for C, that this process is scarcely necessary, a quarter of a tone never occasioning the change of an entire beat in any of our numbers.

399. The intelligent reader cannot but observe, that this system of tuning with perfect IIIds, which is preferred to all others by many great masters, is the one represented by our circular figure of the octave. The IIId is there perfect, and the Vth CG is deficient by a quarter of a comma. We cannot here omit taking notice of a most valuable observation of Dr. Smith's on this temperament, and, in general, on any division of the octave into mean tones and equal limmas.

400. The octave being made up of five mean tones and two limmas, it is plain that, by enlarging the tones, we diminish the limmas, and that the increment of the tone is two-fifths of the contemporaneous diminution of the limma. If, therefore, we employ the symbol v to express any minute variation of this temperament, and make the increment of a mean tone $= 2\,v$, the contemperaneous variation which this induces on a limma will be $= -5\,v$; and if the tone be diminished by the same quantity $-2\,v$, the limma will increase by the quantity $5\,v$. Let us see what are the contemporaneous changes made on all the intervals of the octave when the tone is diminished by $2\,v$.

1. A Vth is made up of three tones and a limma. Therefore the variation of its temperment is $= -6\,v + 5\,v$, or is $= -v$. That is, the Vth is flattened from its former temperament, whatever that may have been, by the quantity $-v$. Consequently the 4th, which is always the complement of the Vth to the octave, has its temperament sharpened by the quantity v.

2. A IId, being a tone distant from the fundamental, has its temperament changed by $-2\,v$.

Therefore a minor 7th is raised by $2\,v$.

3. A minor 3d is made up of a tone and a limma: therefore its variation is $= -2\,v + 5\,v$. $= 3\,v$.

Therefore a major VIth (its complement) loses $-3\,v$.

4. A major IIId, or two tones, has its variation $= -4\,v$

Therefore a minor 6th has its variation $= 4\,v$

5. A major VIIth, the complement of a limma, has $-5\,v$.

6. A tritone, or IVth, must have the variation $= -6\,v$.

Therefore the false 5th must have - - - $6\,v$.

401. From this observation, Dr Smith deduces the following simple mathematical construction: In the strait line CE (Plate V. fig. 2) take the six equal parts C g, g d, d a, a E, E b, b t, and draw through the points of division the six parallel lines g G, d D, &c. Let these lines represent so many scales of the octave, so placed that the points

C, *g*, *d*, &c. may represent the points C, *g*, *d*, &c. of the circular scale in Plate V fig. 1. where it is cut by the dotted lines representing the system of mean tones and limmas. Then, 1*st*, take a certain length *d* G on the first line, to the right hand of the line CE, to represent a quarter of a comma. G will mark the place of the perfect Vth, while *g* represents that of the mean or tempered Vth. 2*dly*, Set off *d* D, double of *g* G, in like manner, to the right hand on the second parallel. This will be the place of the perfect IId to the key note C. 3*dly*, Also set off *a* A, on the third parallel, to the left hand, equal to *g* G. This will mark the place of A, the VIth to the key note C. 4*thly*, Place E on the point *e*, because, in the system of mean tones represented in Plate V. fig. 1. the IIIds were kept perfect. 5*thly*, Make *b* B, to the right hand on the 5th line, equal to *g* G, to mark the place of the perfect VIIth to the key note C. And, 6*thly*, make *t* T, to the right hand on the sixth line, equal to twice *g* G. This will serve for shewing the contemporaneous temperament of the tritone, or IVth, contained between F and B, as also of its complement, the false 5th in Plate V. fig. 1.

It is evident that the temperament of all the notes of the octave, according to the above mentioned system, is properly represented in this figure. The Vth is tempered flat, by the quarter comma G *g*; the IId is tempered flat by the half comma D *d*; the VIth is tempered sharp by a quarter comma A *a*; the IIId is perfect; the VIIth is flat by a quarter comma B *b*; and the 4th is sharp by a quarter comma G *g*.

402. Now, let any other straight line C *t'* be drawn from C across these parallels. This will mark, by the intervals *g'* G, *d'* D. &c. the temperaments of another system of mean tones and limmas. For it is evident, that the contemporaneous variations *g g'*, *d d'*, &c. from the former temperament, are in the just proportions to each other; *g g'* being $= -v$, the variation proper for the Vth, and the opposite temperament for its complement or 4th. In like manner, *a a'* is =

3 v, the variation competent to the VIth; and E e' is = 4 v, the proper variation for the IIId.

In like manner, $b\ b$ is = 5 v, the variation of the VIIth and 2d. And, lastly, $t\ t'$ is the variation o v of the tritone, and its complement, the false fifth.

For all these reasons, any straight line C e' or C e'', drawn from C across the parallels, may justly be called the TEMPERER.

403. This is a very useful construction: For it is plain, that the sounds which can be placed in our organs and harpsichords, which have only twelve keys for an octave, must approach to a system of mean tones. The division of the octave into twelve equal intervals is such a system of mean tones exactly. Now, in such systems, when a line is drawn from C across the parallels, we see, at one glance, not only all the temperaments of the notes with the key note, but also the temperaments of those concords which the notes employed in full harmony make with each other. Thus, in the harmony of K — III — V, the III and V make a minor 3d with each other; and in the harmony of K — 4 — VI, the 4 and VI make a major 3d with each other. Now the reader will easily see, that the first of these concords has its interval diminished on both sides, when the IIId is tempered sharp, but only on one side when it is tempered flat. The mathematical reader will also easily see, that the contemporaneous temperament A a' of the VIth is always equal to the sum g' G and E e', and that A a'' is equal to the difference of g' G and E e'. Therefore the temperament of this subordinate concord, in the full harmony K — III — V, is, in all cases, the same with the contemporaneous temperament of the VIth.

In like manner, he will perceive that the temperament of the subordinate IIId, in the harmony of K — 4 — VI, is equal to the contemporaneous temperament of the III.

We also see, in general, that the whole harmony is more hurt when the temperer lies in the angle ECK, with the IIId

tempered sharp, than when it is in the angle ACE, when the IIId is flat; and that the sum of all the temperaments of the concords with the key is the smallest when the IIIds are perfect. This system of mean tones, with perfect IIIds, would therefore be the best, if the harmony of different concords were equally hurt by the same temperament.

404. We do not know any thing that has been published on the science of music that gives more general and speedy instruction than this simple figure. If it be drawn of such a size as to allow the comma EK to be divided into a number of equal parts, sufficiently sensible, all trouble of calculation will be saved.

We would therefore propose to accompany this figure with proper scales.

The *first* scale should have G g divided into $13\frac{1}{2}$ parts. This will express the logarithmic measures of the temperaments mentioned in § 390, a comma being = 54.

The *second* scale should have g G divided into 36 parts. This gives the beats made in 16 seconds by the notes c, g, when tempered by any quantity G g'.

The *third* scale should have g G divided into 60 parts, for the beats made by the notes c, e, or the notes c, $\bar{a}$.

The *fourth* scale should have g G divided into 72 parts. This gives the beats made by the key note C, with its minor third $e^{\flat}$.

The *fifth* scale should have g G divided into 48 parts, for the beats made by the notes c, f.

The *sixth* scale should have g G divided into 89 parts, on which A a is measured, to get the beats of the subordinate concord formed by g and e in the harmony of K — III — V.

And *lastly*, g G, divided into 80 parts, will give the beats made by f and $\bar{a}$ in the harmony of K — 4 — VI.

405. We are ignorant of the immediate efficient causes of the pleasure we receive from certain consonances, and should therefore receive, with satisfaction, any thing that can help us to approximate to a measure of its degrees. We know

that, in fact, the pleasantness of any individual concord increases as the undulations called *beats* diminish in frequency. It is probable that we shall not deviate very far from the truth, if we suppose the harmoniousness of an individual tempered concord to be proportional to the slowness of these undulations. But it by no means follows, that a tempered Vth and IIId are equally pleasant, each in its kind, when they beat equally slow. There is a difference *in kind* in the pleasures of these concords: and this must arise from the peculiar manner in which the compouent pulses of each concord divide each other. We are certain that this is all the difference that obtains between them in Nature. But the harmoniousness here spoken of is the arrangement which produces this pleasure. We are entitled to say, that this is equal in two given instances, when the arrangements are precisely similar; and when the things arranged are the same, nothing seems to remain in which the instances can differ.

At any rate, it is of consequence to be able to proportion and distribute these undulations at pleasure. They are unpleasant; and when reinforced by uniting, must be more so. The theory puts it in our power to prevent this union; perhaps by making them very unequal; or, if this should give a chance of periodical accumulation, we may find it better to make them all equal. Surely to have all this in our power is very desirable; and this is obtained by the theory of the bats of imperfect consonances.

406. But we are forgetting the process of tuning, and have only tuned three or four notes of our octave. We must tune the rest by considering their relation to notes already tuned. Thus, if *g c* makes 36 beats in 16 seconds, F *c* should make one third less, or about 24 in the same time: because N in the formula is now 160 instead of 240. Proceeding in this way, we shall tune the octave C $\overline{\overline{c}}$ most accurately as a system of mean tones with perfect IIIds, by making the notes beat as follows. A point is put over the note that is to be tuned from the other, and $a+$, or $a-$,

means that the concord is to be tempered sharp or flat. Thus g is tuned from c,

Make	$c\,\dot{g}$ beat	—	36 times in 16 seconds
	$\dot{G}\,c$	+	36
	$G\,\dot{d}$	—	27, *i. e.* $\frac{3}{4}$ths of $g\,e$
	$c\,f$	—	48
	$\dot{c}\,\bar{a}$	+	60 times in 16 seconds
	$c\,\dot{e}$		0, *i. e.* a perfect IIId
	$d\,\dot{f}$ ✻		0
	$e\,\dot{g}$ ✻		0
	$\bar{a}\,\dot{c}$ ✻		0
	$b^{\flat}\,f$ downward	—	24. *i. e.* $\frac{8}{9}$ths of $c\,g$
	$b^{\flat}\,\dot{\bar{b}}^{\flat}$		0, *i. e.* a perfect octave
	$\bar{b}^{\flat}\,\dot{e}^{\flat}$ downward	—	43, *i. e.* $\frac{6}{5}$ths of $c\,g$
	$C\,\dot{\bar{c}}$		0 an octave.

Other processes may be followed, and perhaps some of them better than the process here proposed. Thus, $b^{\flat}$ and $e^{\flat}$ may be tuned as perfect IIIds to d and g downwards. Also, as we proceed in tuning, we can prove the notes, by comparing them with other notes already tuned, &c. &c. &c.

We have directed to tune the two notes $b^{\flat}$ and $e^{\flat}$ by taking the leading Vth downwards. We should have come at the same pipes in the character of a✻ and d✻ in the process of tuning upwards by Vths. But this would not have produced precisely the same sounds, although, in our imperfect instruments, one key must serve for a✻ and $b^{\flat}$. By tuning them as here directed, they are better fitted for the places in which they will be most frequently employed in our usual modulations.

407. It may reasonably be asked, Why so much is sacrificed in order to preserve the IIIds perfect? Were they allowed to retain some part of the sharp temperament that is necessary for preserving the Vths perfect, we should perhaps

improve the harmony. And since enlarging the Vth makes the tone greater, and therefore the limma *mi fa* much smaller, it will bring it nearer to the magnitude of a half tone; and this will be better suited for its double service of the sharp of the note below, and the flat of the note above. Accordingly, such a temperament is in great repute, and indeed is generally practised, although the VIths and the subordinate chords of full harmony are evidently hurt by it. Even Dr. Smith recommends it as well suited to our defective instruments, and gives an extremely easy method of executing it by means of the beats. His method is to make the Vth and IIId beat equally fast, along with the key, the Vth flat, and the third sharp. He demonstrates (on another occasion), that concords beat equally fast with the same bass when their temperaments are inversely as the major terms of their perfect ratios. Therefore draw EG, and divide it in *p*, so that E *p* may be to *p* G as 3 to 5. Then draw C *p*, cutting *g*G in *g'*, and EK in *e'*; and this temperer will produce the temperament we want. It will be found, that E *e'* and G *g'* are each of them 32 of their respective scales.

Therefore make *c g* beat 32 times in 16 seconds

G *c*	32
G *d*	24
G *b*	24, and tune *b b*
d ā	36, and tune *a ā*
d f ✻	36
a e	27
a c ✻	27
e $\overline{b}$	$40\frac{1}{2}$, proving *b* $\overline{b}$
e g ✻	$40\frac{1}{2}$
F *c*	$21\frac{1}{3}$, and tune F *f*
F *a*	$21\frac{1}{3}$, proving *a*
$b^{\flat}$ *f*	$28\frac{1}{2}$, and tune $b^{\flat}$ $\overline{b^{\flat}}$
$e^{\flat}$ $\overline{b^{\flat}}$	$38\frac{1}{2}$
c $\overline{c}$	

It may be proper to add to all these isntructions a caution about the manner of counting the clock while the tuner is counting the beats. If this is to continue for 16 seconds, let the person who counts the clock say *one* at the beat he begins with, and then telling them over *to himself*, let him say *done* instead of 17. Thus 16 intervals will elapse while the tuner is counting the beats. Were he to begin to count at *one*, and stop when he hears sixteen, he would get the number of beats in 15 seconds only.

408. We do not hesitate to say, that this method of tuning by beats is incomparably more exact than by the mere judgment of the ear. We cannot mistake more than one beat. This mistake in the concord of the Vth amounts to no more than $\frac{1}{1080}$th of a comma; and in the IIId it is only $\frac{1}{1080}$.

409 It may be objected that it is fit only for the organ and instruments of continued sounds, but will not do for the quickly perishing sounds of the harpsichord. True, it is the only method worthy of that noble instrument, and this alone is a title to high regard. But farther; the accuracy attainable by it, renders it the only method fit for the examination of systems of temperament. Even for the harpsichord it is much more exact, and more certain in its process, than any other. It does not proceed, by a random trial of a flattened series of Vths, and a comparison with the resulting IIId, and a second trial, if the first be unsatisfactory. It says at once, let the Vth beat so many times in 16 seconds. Even in the second method, without counting, and merely by the equality of the beats of the Vth and IIId, the progress is easy Both are tuned perfect. The Vth is then flattened a little, and the IIId sharpened;—if the Vth beat faster than the IIId, alter it first.

All difficulty is obviated by the simple contrivance of a variable pendulum, already described. This may be made exact by any person that will take a little pains; and when once made, will serve for every trial. When the ball is set

to the proper number, and the pendulum set a swinging, we can come very near the truth by a very few trials.

N. B. In tuning a piano forte, which has always two strings to a key, we must never attempt tuning them both at once; the back unison of both notes of the concord must be damped, by sticking in a bit of soft paper behind it.

We hope that the instructions now given, and the application of them to two very respectable systems of temperament, are sufficient for enabling the attentive reader to put this method of tuning successfully in practice, and that he perceives the efficiency of it for attaining the desired end. But before we take leave of it, we beg leave to mention another circumstance, which evinces the just value of the general theory of the beats of imperfect consonances as delivered by Dr. Smith.

410. These reinforcements of sound, which are called *beatings*, are noises. If any noise whatever be repeated, with sufficient frequency, at equal intervals, it becomes a musical note, of a certain determinate pitch. If it recur 60 times in a second, it becomes the note C *fa ut*, or the double octave below the middle C of our harpsichords, or the note of an open pipe eight feet long. Now there is a similar (we may call it the very same) reinforcement of sound in every concord. Where the pulse of one sound of the concord bisects the pulse of the other, the two sounds are more uniformly spread: but where they coincide, or almost coincide, the condensation of one undulation combines with that of the other, and there comes on the ear a stronger condensation, and a louder sound. This may be called a *noise*; and the equable and frequent recurrence of this noise should produce a musical note. If, for instance, *c* and *a* are sounded together: There is this noise at every third pulse of *c*, and every fifth pulse of *a*; that is, 80 times in a second. This should produce a note which is a 12th below *c*, and a 17th major below *a*; that is, the double octave below *f*, which makes 320 vibrations in a second. That is to say, along

with the two notes *c* and *a* of the concord, and the compound sound, which we call the *concord of the VIth*, we should hear a third note FF in the bass. Now this is known to be a fact, and it is the grave harmonic observed by Romieu and Tartini about the year 1754, and verified by all musicians since that time. Tartini prized this observation as a most important discovery, and considered it as affording a foundation for the whole science of music. We see that it is all included in the theory of beats published five years before, namely, in 1749; and every one of these grave harmonics, or Tartinian sounds, as they have been called, are immediate consequences of this theory, The system of harmonious composition which Tartini has, with wonderful labour and address, founded on it, has therefore no solidity. It is, however, preferable to Rameau's, because it proceeds on a fact founded on the nature of musical sounds; whereas Rameau's is a mere whim, proceeding on a false assumption; namely, "that a musical sound is essentially accompanied by its octave, 12th, and 17th *in alto*." This is not true, though such accompaniment be very frequent, and it be very difficult to prevent it. Mr. Rameau ought to have seen this. Are these acute harmonics musical sounds or not? He surely will not deny this. Therefore they, too, are essentially accompanied by *their* harmonics, and this absolutely and necessarily *ad infinitum*; which is certainly absurd. We shall have a better occasion for considering this point when we describe the *Trumpet Marigni*.

411. We have taken notice of only two systems of temperament; both of them are systems of mean tones, and are in good repute as practicable methods. It would be almost an endless task to mention all the systems of temperament which have been proposed. Dr. Smith, after having, with great ingenuity, appreciated the changes of harmoniousness that are induced on the different concords by the same temperament, and having assigned that proportion of tempera-

ment which renders them equally harmonious, each in its kind, gives a system of temperament which he calls EQUAL HARMONY. Each concord, (excepting the octave) is tempered in the inverse proportion of the product of the terms of its perfect ratio. It is very nearly equivalent to a division of the octave into 50 equal parts. We do not give any farther account of it here, although we think its harmony preferable to any thing that we have ever heard. We heard it, as executed for him, and under his inspection, by the celebrated harpsichord-maker Kirkmann, both when the instrument was yet in the hands of the maker, and afterwards by the ingenious author. We have also heard some excellent musicians declare, that the organ of Trinity college chapel at Cambridge was greatly improved in its harmony by the change made on its temperament under the inspection of Dr. Smith. When we name Stanley, we presume that the authority will not be disputed. We mention this, because the writer in the Philosophical Transactions speaks of this system, with flattened major thirds, as of no value. But we do not give any farther account of it, because it is not suited to our instruments, which have but twelve sounds in the octave.

412. The reader will please to recollect, that the great object of temperament is twofold. First, to enable us to transpose music from one pitch to another, so that we may make any note of the organ the fundamental of the piece. This undoubtedly requires a system approaching to one of mean tones, because the harmony must be the same in every key. This requires temperament, because a sound must be occasionally considered, either as the sharp of the note below it, or the flat of the one above. This cannot produce perfect harmony, because the limma of the perfect diatonic scale is greater than a half tone. Thus a temperament is necessary merely for the sake of the melody. But, *secondly*, the nature of modern music requires every note to be accompanied, or considered as accompanied, with full harmony. This is, in

fact, the same thing with modulating on every different note as a fundamental; but it requires a much closer attention to the perfection of the intervals, because a defect or excess in an interval that would scarcely offend the ear, if the notes were heard in succession, is quite intolerable when they are sounded together. Here the difference between the major and minor tone is of almost as great moment as the difference of the limma from a semitone. The second object, therefore, is to obtain, in the compass of three octaves, as many good concords of full harmony; that is, consisting of a fundamental with its major third and its fifth, erect or inverted, as possible. There is no other harmony, although our notes have frequently a different situation and appearance.

413. It is no wonder that, in a subject where we are yet to seek for a principle, the attempts to attain this object have been very various, and very gratuitous. The mathematicians, even in modern times, have allowed themselves to be led away by fancies about the simplicity and consequent perfection of ratios; and having no clear principle, it is no wonder that some of their deductions are contrary to experience. According to Euler, those ratios which are most perfect, that is, most simple, admit of least temperament. The octave is therefore infinitely perfect; for it is allowed by all, that it must not have the smallest temperament. A Vth must be less tempered than a IIId. Even the pract ical mu sician thinks that he has tempered these two concords equally, when the offensive quality of each is made equally so; but in this case it is demonstrable, that the Vth has been much more tempered than the IIId. But this could not be discovered till we got the theory of beats.

Most of the mathematical musicians adhered to systems of mean tones; or, which are equivalent to such systems, gving similar harmonies on every key of the harpsichord. This is surely the most natural, and is peculiarly suggestedby the transposing of music from one pitch to another: but they differ exceedingly, and without giving any convin-

g arguments in their estimation of the effe cts of the same

temperament on different concords. Much of this, we apprehend, arises from disposition. Persons of a gay disposition relish the harmony of the IIId, and prefer a sharp to a flat temperament of this concord. Persons of a more pensive disposition, prefer such temperaments as allow the minor thirds to be more perfect.

414. But there are many, eminent both as performers and as theorists, who reject any system which gives the same harmonies on every note of the octave. They observe, that in the progress of the cultivation of music in Europe, the melodies of all nations have gradually approached to a certain uniformity. Certain cadences, closes, strains, and phrases, are becoming every day more common; and even in the conduct of a considerable piece of music, and the gradual but slow passage of the modulation from one key into another, there is a certain regularity. Nay, they add, that this cannot be greatly deviated from without becoming very offensive. We may remain ignorant of the cause of this uniformity; but its existence seems to prove that it arises from some natural principle; and therefore it ought to be complied with, and our temperaments should be accommodated to it. The result of this uniformity in the music of our times is, that the modulation on some keys is much less frequent than on others, and this frequency decreases in a certain order. Supposing that we begin on C. A piece of plain music seldom goes farther than G and F. A little more fancy and refinement leads the composer into D, or into B♭, &c. &c. It would therefore be desirable to adjust our temperaments so, that the harmonies in C shall be the best possible, and gradually less perfect in the order of modulation. Thus we shall, in our general practice, have finer harmony than if it were made equal throughout the octave; because the unavoidable imperfections are thrown into the least frequented places of the scale. The practical musicians add to this, that by such a temperament the different keys acquire characters, which fit each of them more particularly

for the expression of different sentiments, and for exciting different emotions. This is very perceptible in our harpsichords as they are generally tuned. The major key of A is remarkably brilliant; that of F is as remarkably simple, &c.

We cannot say that we are altogether convinced by these arguments. The violin is unquestionably the instrument of the greatest powers. A concert of instruments of this kind, unembarrassed by the harpsichord, or any instruments incapable of occasional temperament, is the finest music we have. The performers make no such degradations of harmony, but keep it as perfect as possible throughout; and a violin performer is sensible of violence of constraint when he accompanies a keyed instrument into these unfrequented paths. Let him play the same music alone, and he will play it quite differently, and much more to his own satisfaction. We imagine, too, that much of the uniformity spoken of is the result of imitation and fashion, and even of the temperaments that we have preferred. There is an evident distinction in the native music of different nations. An experienced musician will know, from a few bars, whether an air is Irish, Scotch, or Polish. This distinction is in the modulation, which, in those nations, follows different courses, and should therefore, on the same principle, lead to different temperaments.

With respect to the variety of characters given to the different keys, we must acknowledge the fact. We have tuned a piano forte in the usual manner; but instead of beginning the process with C, we began it with D. An excellent performer of voluntaries sat down to the instrument, and began to indulge his rich fancy; but he was confounded at every step: he thought the instrument quite out of tune. But when he was informed how it had been tuned, and then tried a known plain air on it, he declared it to be perfectly in tune. It is still very doubtful, however, whether we should not have much finer music, by equalising the har-

mony in the different keys, and trusting for the different expression so much spoken of to a judicious mixture of other notes called *discords*.

415. After all, the great uncertainty about the most proper temperament has remained so long undetermined, because we had no method of executing with certainty any temperament that was offered to the public. What signifies it on what principle it may be proper to flatten a Vth one-fifth of a comma, and sharpen a VIth one-seventh of a comma, unless we are able to do both the one and the other? Till Dr. Smith published the theory of beats, the monochord was the only assistance we had: but however nicely it may be divided, it is scarcely possible to make the moveable bridge so steady and so accurate in its motion, that it will not sensibly derange the tension of the string. We have seen some very nice and costly monochords; but not one of them could be depended on to one-eight of a comma. Even if perfect, they gave but momentary sounds by pinching. The bow cannot be trusted, because its pressure changes the tension. Mr. Watt's experiments with his monochord of continued sound shewed this evidently. A pitch-pipe with a sliding piston promises the greatest accuracy; but we are sadly disappointed, because the graduation of the piston cannot be performed by any mathematical rule. It must be pushed more than half way down to produce the octave, more than one-third to produce the Vth, &c. and this without any rule yet discovered. Thanks to Dr. Smith we can now produce an instrument tuned exactly, according to any proposed system, and then submit it to the fair examination of musicians. Even the speculatist may now form a pretty just opinion of the merits of a system, by calculating, or measuring by such scales as we have proposed, the beats produced by the tempered concords in all parts of the octave. No one who has listened with attention to the rattling beats of a full organ, with its twelfth and sesquialter stops all sounding, will deny that they are hostile to

all harmony or good music. We cannot be much mistaken in preferring any temperament in proportion as it diminishes the number of those beats We should therefore examine them on this principle alone; attending more particularly to the beats of the third major, because these are in fact the loudest and most disagreeable; and we must not content ourselves with the beats of each concord with the fundamental of the full harmony, whether K—III—V, or K—4—VI, or K—3—V, or K—4—6, which sometimes occurs. We must attend equally to the beats of the two notes of accompaniment with each other: these are generally the most faulty.

416. This examination is neither difficult nor tedious. 1. Write down, in one column, the lengths of the strings or divisions of the monochord. 2. In another write their logarithms; in a third the remainders, after subtracting each from the logarithm of the fundamental. 3. Have at hand a similar table for the perfect diatonic scale. 4. Compare these, one by one, and note the difference, + or —, in a 4th column. These are the temperaments of each note of the scale. 5. Compare every couple of notes which will compose a major or minor third, or a fifth, by subtracting the logarithm of the one note from that of the other. The differences are the intervals tempered. 6. Compare these with the perfect intervals of the diatonic scale, and note the differences, + or —, and set them down in a fifth column. These are all the temperaments in the system. 7. If we have used logarithms consisting of five decimal places, which is even more than sufficient, consider these numeral temperaments as the q of the formula given in § 392, for calculating the beats, and then p is always $= 540$. Or we may make another column, in which the temperaments are reduced to some easy fraction of a comma.

417. We shall content ourselves with giving one example; the temperament proposed by Mr. Young in the Philosophi-

cal Transactions for 1800. It is contained in the following table:

1.	2.	3.	4.	5.		
				IIIds upward on		
C	100000	5.00000		C	135	Sharp.
C♯	94123	4.97645	2355	G. F	190	Sharp.
D	89304	4.95087	4913	D. B♭	245	Sharp.
E♭	83810	4.92330	7670	A. E♭	346	Sharp.
E	79752	4.90174	9826	E. A♭	448	Sharp.
F	74921	4.87461	12539	B. C♯	494	Sharp.
F♯	71041	4.85151	14849	F♯	540	Sharp.
G	66822	4.82492	17508	3ds upward on		
G♯	63148	4.80036	19964	A. E.	236	Flat.
A	59676	4.77580	22420	D. B.	291	Flat.
B	56131	4.74921	25079	G. F♯	346	Flat.
B	53224	4.72610	27390	C. C♯	448	Flat.
C	50000	4.69897	30103	F. G♯	494	Flat.
				B♭ E♭	540	Flat.

Vths upward on

E♭. G♯. C♯. F♯	perfect		
F. B♭. E. B	46	Flat.	
C. G. D. A	116	Flat.	

Interval of a comma	540
minor third	7918
major third	9691
fifth	17609

The first column of the above table contains the ordinary designations of the notes. The second contains the corresponding lengths of the monochord. The third contains the logarithms of column second. The fourth contains the difference of each logarithm from the first. The next column contains, first, the temperaments of all the major thirds, having for their lowest note the sound corresponding to the letter. Thus 494, or $\frac{494}{540}$ of a comma, is the temperament of the IIId, B — D♯, and C♯— F. Secondly, it contains all the minor thirds formed on the notes represented by the letters. The column below contains the temperaments of

the Vths. *N. B.* These temperaments are calculated by the author. We have found some of them a little different. Thus we make the temperament of C — G only 108. Below this we have set down the measures of the perfect intervals, which are to be compared with the differences of the logarithms in column third.

418. We presume not to decide on the merits of this temperament: Only we think that the temperaments of several thirds, which occur very frequently, are much too great; and many instances of the 6th, which is frequent in the flat key, are still more strongly tempered. A temperament however, which very nearly coincides with Dr. Young's, has great reputation on the continent. This is the temperament by Mr. Kirnbergher, published at Berlin in 1771, in his book called *Die Kunst des reinen Satzes in der Musik.* The eminent mathematician Major Templehoff has made some important observations on this temperament, and on the subject in general, in an essay published in 1775, Berlin. Dr. Young's is certainly preferable.

The monochord is thus divided by Kirnbergher:

C =	1,0000	F =	7500	B♭ =	5625
C𝄪	9492	F𝄪	7111	B	5313
D	8889	G	6667	*c*	5000
E♭	8437	G𝄪	6328		
E	8000	A	5963		

We conclude this article (perhaps too long) by earnestly recommending to persons who are not mathematically disposed, the sliding scales, either circular or rectilineal, containing the octave divided into 301 parts; and a drawing of Plate VI. fig. 2. on card paper, of proper size, having the quarter-comma about two inches, and a series of scales corresponding to it. This will save almost the whole of the calculation that is required for calculating the beats, and for examining temperaments by this test. To readers of more information, we earnestly recommend a careful perusal of Smith's Harmonics, second edition. We acknowledge a

great partiality for this work, having got more information from it than from all our patient study of the most celebrated writings of Ptolemy, Huyghens, Euler, &c. It is our duty also to say, that we have got more information concerning the music of the Greeks from Dr. Wallis's appendix to his edition of Porphyrius's Commentary on Ptolemy's Harmonics, than from any other work.

TRUMPET.

Some Greek historians ascribe the invention of the trumpet to the Tyrrhenians; but others, with greater probability, to the Egyptians; from whom it might have been transmitted to the Israelites. The trumpet was not in use among the Greeks at the time of the Trojan war; though it was in common use in the time of Homer. According to Potter (*Arch. Græc.* vol. ii. cap. 9.), before the invention of trumpets, the first signals of battle in primitive wars were lighted torches; to these succeeded shells of fishes, which were sounded like trumpets. And when the trumpet became common in military use, it may well be imagined to have served at first only as a rough and noisy signal of battle, like that at present in Abyssinia and New Zealand, and perhaps with only one sound. But, even when more notes were produced from it, so noisy an instrument must have been an unfit accompaniment for the voice and poetry; so that it is probable the trumpet was the first solo instrument in use among the ancients.

The *Articulate* TRUMPET, comprehending both the *speaking* and the *hearing* trumpet, is by much the most valuable instrument, and has, in one of its forms, been used by people among whom we should hardly have expected to find such improvements.

That the *speaking trumpet*, of which the object is to increase the force of articulate sounds, should have been known to the ancient Greeks, can excite no wonder; and therefore we easily admit the accounts which we read of the horn or trumpet, with which Alexander addressed his army, as well as of the whispering caverns of the Syracusan tyrant. But that the natives of Peru were acquainted with this instrument, will probably surprise many of our readers. The fact however seems incontrovertible.

In the History of the Order of Jesuits, published at Naples in 1601 by Beritaria, it is said, that in the year 1595, a small convent of that order in Peru, situated in a remote corner, was in danger of immediate destruction by famine. One evening the superior Father Samaniac, implored the help of the cacique; next morning, on opening the gate of the monastery, he found it surrounded by a number of women, each of whom carried a small basket of provisions. He returned thanks to Heaven for having miraculously interposed, by inspiring the good people with pity for the distress of his friars. But when he expressed to them his wonder how they came all to be moved as if by mutual agreement with these benevolent sentiments, they told him it was no such thing; that they looked on him and his countrymen as a pack of infernal magicians, who by their sorceries had enslaved the country, and had bewitched their good cacique, who hitherto had treated them with kindness and attention, as became a true worshipper of the sun; but that the preceding evening at sunset, he had ordered the inhabitants of such and such villages, about six miles off, to come that morning with provisions to this nest of wizzards.

The superior asked them in what manner the governor had warned so many of them in so short a time, at such a distance from his own residence? They told him that it was by the trumpet: and that every person heard at his own door the distinct terms of the order. The father had heard nothing; but they told him that none heard the trumpet but the inhabitants of villages to which it was directed. This is a piece of very curious information; but, after allowing a good deal to the exaggeration of the reverend Jesuits, it cannot, we think, be doubted, but that the Peruvians actually possessed this stentorophonic art. For we may observe that the effect described in this narration resembles what we *now know* to be the effects of speaking trumpets, while it is unlike what the inventor of such a tale would naturally and ignorantly say. Till speaking trumpets were really known, we should expect the sound to be equally diffused on all sides, which is not the case; for it is much stronger in the line of the trumpet than in any direction very oblique to it.

About the middle of the last century, Athanasius Kircher turned his attention to the philosophy of sound, and in different works threw out many useful and scientific hints on the construction of speaking trumpets, but his mathematical illustrations were so vague, and his own character of inattention and credulity so notorious, that for some time these works did not attract the notice to which they were well entitled.

About the year 1670 Sir Samuel Morland, a gentleman of great ingenuity, science, and order, took up the subject, and proposed as a question to the Royal Society of London, What is the best form for a speaking trumpet? which he called a stentorphonic horn. He accompanied his demand with an account of his own notions on the subject (which he acknowledged to be very vague and conjectural), and an exhibition of some instruments constructed according to his views. They were in general very large conical tubes, sud-

denly spreading at the very mouth to a greater width. Their effect was really wonderful. They were tried in St. James's park; and his Majesty K. Charles II. speaking in his ordinary colloquial pitch of voice through a trumpet only $5\frac{1}{2}$ feet long, was clearly and most distinctly heard at the distance of a thousand yards. Another person, selected we suppose for the loudness and distinctness of his voice, was perfectly understood at the distance of four miles and a half. The fame of this soon spread; Sir Samuel Morland's principles were refined, considering the novelty of the thing, and differ considerably from Father Kircher's. The aerial undulations (for he speaks very accurately concerning the nature of sound) endeavour to diffuse themselves in spheres, but are stopped by the tube, and therefore redundulate towards the axis like waves from a bank, and, meeting in the axis, they form a strong undulation a little farther advanced along the tube, which again spreads, is again reflected, and so on, till it arrives at the mouth of the tube greatly magnified, and then it is diffused through the open air in the same manner, as if all proceeded from a *very* sonorous point in the centre of the wide end of the trumpet. The author distinguishes with great judgment between the prodigious reinforcement of sound in a speaking trumpet and that in the musical trumpet, bugle-horn, conch shell, &c.; and shows that the difference consists only in the violence of the first sonorous agitation, which can be produced by us only on a very small extent of surface. The mouth-piece diameter therefore of the musical trumpet must be very small, and the force of blast very considerable. Thus one strong but simple undulation will be excited, which must be subjected to the modifications of harmony, and will be augmented by using a conical tube*. But a speaking trumpet must make

* Accordingly the sound of the bugle-horn, of the musical trumpet, or the French horn, is prodigiously loud, when we consider the small passage through which a moderate blast is sent by the trumpeter.

no change on the nature of the first undulations; and each point of the mouth-piece must be equally considered as the centre of sonorous undulations, all of which must be reinforced in the same degree, otherwise all distinctness of articulation will be lost. The mouth-piece must therefore take in the whole of the mouth of the speaker.

When Sir Samuel Morland's trumpet came to be generally known on the continent, it was soon discovered that the speaker could be heard at a great distance only in the line of the trumpet; and this circumstance was by a Mr. Cassegrain (*Journ. des Sçavans* 1672, p. 131.) attributed to a defect in the principle of its construction, which he said was not according to the laws of sonorous undulations. He proposed a conoid formed by the revolution of a hyperbola round its assymptote as the best form. A Mr. Hase of Wirtemberg, on the other hand, proposed a parabolic conoid, having the mouth of the speaker placed in the focus. In this construction he plainly went on the principle of a reflection similar to that of the rays of light; but this is by no means the case. The effect of the parabola will be to give one reflection, and in this all the circular undulations will be converted into plane waves, which are at right angles to the axis of the trumpet. But nothing hinders their subsequent diffusion; for it does not appear that the sound will be enforced, because the agitation of the particles on each wave is not augmented.

The subject is exceedingly difficult. We do not fully comprehend on what circumstance the affection or agitation of our organ, or simply of the membrana tympani, depends. A more violent agitation of the same air, that is, a wider oscillation of its particles, cannot fail to increase the impulse on this membrane. The point therefore is to find what concourse of feeble undulations will produce or be equivalent to a great one. The reasonings of all these restorers of the speaking trumpet are almost equally specious, and each points out some phenomenon which should characterise the princi-

ple of construction, and thus enable us to say which is most agreeable to the procedure of nature.—Yet there is hardly any difference in the performance of trumpets of equal dimensions made after these different methods.

The propagation of light and that of elastic undulations seem to require very different methods of management. Yet the ordinary phenomena of echoes are perfectly explicable by the acknowledged laws either of optics or acoustics; still however there are some phenomena of sound which are very unlike the genuine results of elastic undulations. If sounds are propagated spherically, then what comes into a room by a small hole should diffuse itself from that hole as round a centre, and it should be heard equally well at twelve feet distance from the hole in every direction. Yet it is very sensibly louder when the hearer is in the straight line drawn from the sonorous body through the hole. A person can judge of the direction of the sounding body with tolerable exactness. Cannon discharged from the different sides of a ship are very easily distinguished, which should not be the case by the Newtonian theory; for in this the two pulses on the ear should have no sensible difference.

The most important fact for our purpose is this: An echo from a small plane surface in the midst of an open field is not heard, unless we stand in such a situation that the angle of reflected sound may be equal to that of incidence. But by the usual theory of undulations, this small surface should become the centre of a new undulation, which should spread in all directions. If we make an analogous experiment on watery undulations, by placing a small flat surface so as to project a little above the water, and then drop in a small pebble at a distance, so as to raise one circular wave, we shall observe, that when this wave arrives at the projecting plane, it is disturbed by it, and this disturbance spreads from it on all sides. It is indeed sensibly stronger in that line which is drawn from it at equal angles with the line drawn to the place where the pebble was dropped. But in the

case of sound, it is a fact, that if we go to a very small distance on either side of the line of reflection, we shall hear nothing.

Here then is a fact, that whatever may be the nature of the elastic undulations, sounds are reflected from a small plane in the same manner as light. We may avail ourselves of this fact as a mean for enforcing sound, though we cannot explain it in a satisfactory manner. We should expect from it an effect similar to the hearing of the original sound, along with another original sound coming from the place from which this reflected sound diverges. If therefore the reflected sound or echo arrives at the ear in the same instant with the original sound, the effect will be doubled; or at least it will be the same with two simultaneous original sounds. Now we know that this is in some sense equivalent to a stronger sound. For it is a fact, that a number of voices uttering the same or equal sounds are heard at a much greater distance than a single voice. We cannot perhaps explain how this happens by mechanical laws, nor assign the exact proportion in which 10 voices exceed the effect of one voice; nor the proportion of the distances at which they seem equally loud. We may, therefore, for the present, suppose that two equal voices at the same distance are twice as loud, three voices three times as loud, &c. Therefore if, by means of a speaking trumpet, we can make 10 equal echoes arrive at the ear at the same moment, we may suppose its effect to be to increase the audibility 10 times; and we may express this shortly, by calling the sound 10 times louder or more intense.

But we cannot do this precisely. We cannot by any contrivance make the sound of a momentary snap, and those of its echoes, arrive at the ear in the same moment, because they come from different distances. But if the original noise be a continued sound, a man's voice, for example, uttering a continued uniform tone, the first echo may reach the ear at the same moment with the second vibration of

the larynx; the second echo along with the third vibration, and so on. It is evident that this will produce the same effect. The only difference will be, that the articulations of the voice will be made indistinct, if the echoes come from very different distances. Thus if a man pronounce the syllable *taw*, and the 10 successive echoes are made from places which are 10 feet farther off, the 10th part of a second (nearly) will intervene between hearing the first and the last. This will give it the sound of the syllable *thaw*, or perhaps *raw*, because *r* is the repetition of *t*. Something like this occurs when, standing at one end of a long line of soldiers, we hear the muskets of the whole line discharged in one instant. It seems to us the sound of a running-fire.

The aim therefore in the construction of a speaking trumpet may be, to cause as many echoes as possible to reach a distant ear without any perceptible interval of time. This will give distinctness, and something equivalent to loudness. *Pure* loudness arises from the violence of the single aerial undulation. To increase this may be the aim in the construction of a trumpet; but we are not sufficiently acquainted with the mechanism of these undulations to bring this about with certainty and precision; whereas we can procure this accumulation of echoes without much trouble, since we know that echoes are, *in fact*, reflected like light. We can form a trumpet so that many of these lines of reflected sound shall pass through the place of the hearer. We are indebted to Mr. Lambert of Berlin for this simple and popular view of the subject; and shall here give an abstract of his most ingenious Dissertation on Acoustic Instruments, published in the Berlin Memoirs for 1763.

Sound naturally spreads in all directions; but we know that echoes or reflected sounds proceed almost strictly in certain limited directions. If therefore we contrive a trumpet in such a way that the lines of echo shall be confined within a certain space, it is reasonable to suppose that the sound will become more audible in proportion as this diffusion is

prevented. Therefore if we can oblige a sound which, in the open air, would have diffused itself over a hemisphere, to keep within a cone of 120 degrees, we should expect it to be twice as audible within this cone. This will be accomplished, by making the reflections such that the lines of reflected sound shall be confined within this cone. *N. B.* We here suppose that nothing is lost in the reflection. Let us examine the effect of a cylindrical trumpet.

Let the trumpet be a cylinder ABED (Plate VI. fig. 1.) and let C be a sounding point in the axis. It is evident that all the sound in the cone BCE will go forward without any reflection. Let CM be any other line of sound, which we may for brevity's sake call a *sonorous or phonic line.* Being reflected in the points M, N, O, P, it is evident that it will at last escape from the trumpet in a direction PQ, equally diverging from the axis with the line CM. The same must be true of every other sonorous line. Therefore the echoes will all diverge from the mouth of the trumpet in the same manner as they would have proceeded from C without any trumpet. Even supposing, therefore, that the echoes are as strong as the original sound, no advantage is gained by such a trumpet, but that of bringing the sound forward from C to *c*. This is quite trifling when the hearer is at a distance. Yet we see that sounds may be heard at a very great distance, at the end of long, narrow, cylindrical, or prismatical galleries. It is known that a voice may be distinctly heard at the distance of several hundred feet in the Roman aqueducts, whose sides are perfectly straight and smooth, being plastered with stucco. The smooth surface of the still water greatly contributes to this effect. Cylindrical or prismatical trumpets must therefore be rejected.

Let the trumpet be a cone BCA (Plate VI. fig. 2.) of which CN is the axis, DK a line perpendicular to the axis, and DFHI the path of a reflected sound in the plane of the axis. The last angle of reflection IHA is equal to the last angle of incidence FHC. The angle BFH, or its equal CFD, is

equal to the angles FHD and FCH; that is, the angle of incidence CFD exceeds the next angle of incidence FHC, by the angle FCD; that is by the angle of the cone. In like manner, FDH exceeds CFD by the same angle FCD. Thus every succeeding angle, either of incidence or reflection, exceeds the next by the angle of the cone. Call the angle of the cone a, and let b be the first angle of incidence PDC. The second, or DFC, is $b - a$. The third, or FHC, is $b - 2\,a$, &c.: and the nth angle of incidence or reflection is $b - n\,a$, after n reflections. Since the angle diminishes by equal quantities at each subsequent reflection, it is plain, that whatever be the first angle of incidence, it may be exhausted by this diminution; namely, when n times a exceeds or is equal to b. Therefore to know how many reflections of a sound, whose first incidence has the inclination b can be made in an infinitely extended cone, whose angle is a, divide b by a; the quotient will give the number n of reflections, and the remainder, if any, will be the last angle of incidence or reflection less than a. It is very plain, that when an angle of reflection IHA is equal to or less than the angle BCA of the cone, the reflected line HI will no more meet with the other side CB of the cone.

We may here observe, that the greatest angle of incidence is a right angle, or 90°. This sound would be reflected back in the same line, and would be incident on the opposite side in an angle $= 90° - a$, &c.

Thus we see that a conical trumpet is well suited for confining the sound: for by prolonging it sufficiently, we can keep the lines of reflected sound wholly within the cone. And when it is not carried to such a length as to do this, when it allows the sounding line GH, for example, to escape without farther reflection, the divergency from the axis is less than the last angle of reflection BFH by half the angle BCA of the cone. Let us see what is the connection between the length and angle of ultimate reflection.

We have sin. $\overline{b-a}$: sin. b = CD : CF, and CF = CD $\times \frac{\text{sin. } b}{\text{sin. } \overline{b-a}}$, and sin. $\overline{b-2a}$: sin. $\overline{b-a}$ = CF : CH, and

$$\text{CH} = \text{CF} \times \frac{\text{sin. } b-a}{\text{sin. } \overline{b-2a}} \quad \text{CD} \times \frac{\text{sin. } b}{\text{sin. } \overline{b-a}} \times \frac{\text{sin. } b-a}{\text{sin. } \overline{b-2a}},$$

$$= \text{CD} \times \frac{\text{sin. } b}{\text{sin. } -2a}, \text{ \&c.}$$

Therefore if we suppose X to be the length which will give us n reflections, we shall have $\text{X} = \text{CD} \times \frac{\text{sin. } b}{\text{sin. } \overline{b-na}}$.

Hence we see that the length increases as the angle $\overline{b-na}$ diminishes; but is not infinite, unless na is equal to b. In this case, the immediately preceding angle of reflection must be a, because these angles have the common difference a. Therefore the last reflected sound was moving parallel to the opposite side of the cone, and cannot again meet it. But though we cannot assign the length which will give the nth reflection, we can give the length which will give the one immediately preceding, whose angle with the side of the cone is a. Let Y be this length. We have $\text{Y} = \text{CD} \times \frac{\text{sin. } b}{\text{sin. } a}$. This length will allow every line of sound to be reflected as often, saving once, as if the tube were infinitely long. For suppose a sonorous line to be traced backwards, as if a sound entered the tube in the direction $i\,h$, and were reflected in the points h, f, d, &c., D, the angles will be continually augmented by the constant angle a. But this augmentation can never go farther than $90° + \frac{1}{2}a$. For if it reaches that value at D, for instance, the reflected line DK will be perpendicular to the axis CN; and the angle ADK will be equal to the angle DKB, and the sound will come out again. This remark is of importance on another account.

Now suppose the cone to be cut off at D by a plane perpendicular to the axis, KD will be the diameter of its mouth piece and if we suppose a mouth completely occu-

pying this circle, and every point of the circle to be sonorous, the reflected sounds will procced from it in the same manner as light would from a flame which completely occupies its area, and is reflected by the inside of the cone. The angle FDA will have the greatest possible sine when it is a right angle, and it never can be greater than ADK, which is $= 90 + \frac{1}{2}a$. And since between $90° + \frac{1}{2}a$, and $90 - \frac{1}{2}a$, there must fall some multiple of a; call this multiple b. Then, in order that every sound may be reflected as often as possible, saving once, we must make the length of it $X = CD \times \frac{S, b}{S, a}$.

Now since the angle of the cone is never made very great, never exceeding 10 or 12 degrees, b, can never differ from 90 above a degree or two, and its sine cannot differ much from unity. Therefore X will be very nearly equal to $\frac{CD}{S, a}$, which is also very nearly equal to $\frac{CD}{2\,S, \frac{1}{2}a}$; because a is small, and the sines of small arches are nearly equal and proportional to the arches themselves. There is even a small compensation of errors in this formula. For as the sine of 90° is somewhat too large, which would give X too great, 2 S, $_2$ a is also larger than the sine of a. Thus let a be 12°: then the nearest multiple of a is 84 or 96°, both of which are as far removed as possible from 90°, and the error is as great as possible, and is nearly $\frac{1}{1000}$th of the whole.

This approximation gives us a very simple construction. Let CM be the required length of the trumpet, and draw ML perpendicular to the axis in O. It is evident that S, MCO : rad. $=$ MO : CM, and CM; or $X = \frac{MO}{S, \frac{1}{2}a}$, $= \frac{LM}{2\,S, \frac{1}{2}a}$, but $X = \frac{CD}{2\,S, \frac{1}{2}a}$, and therefore LM is equal to CD.

If therefore the cone be of such a length, that its diameter at the mouth is equal to the length of the part cut off, every line of sound will have at least as many reflections, save one, as if the cone were infinitely long; and the last reflected line will either be parallel to the opposite side of the cone, or lie nearer the axis than this parallel; consequently such a cone will confine all the reflected sounds within a cone whose angle is $2a$, and will augment the sound in the proportion of the spherical base of this cone to a complete hemispherical surface. Describe the circle DKT round C, and making DT an arch of 90, draw the chord DT. Then since the circles described with the radii DK, DT, are equal to the spherical surfaces generated by the revolution of the arches DK and DKT round the axis CD, the sound will be condensed in the proportion of DK^2 to DT^2.

This appears to be the best general rule for constructing the instrument; for, to procure another reflection, the tube must be prodigiously lengthened, and we cannot suppose that one reflection more will add greatly to its power.

It appears, too, that the length depends chiefly on the angle of the cone; for the mouth-piece may be considered as nearly a fixed quantity. It must be of a size to admit the mouth when speaking with force and without constraint. About an inch and a half may be fixed on for its diameter. When therefore we propose to confine the sound to a cone of twice the angle of the trumpet, the whole is determined by that angle. For since in this case LM is equal to CD, we have $DK : CD = LM$ (or CD) $: CM$ and $CM = \frac{CD^2}{DK}$.

But $2 S, \frac{1}{2} a : 1 = DK : CD$,
and $2 S, \frac{1}{2} a : 1 = CD : CM$;
therefore $4 S, {}^2\frac{1}{2} a : 1 = DK : CM$,
And $CM = \frac{DK}{4 S, {}^2\frac{1}{2} a}, = \frac{DK}{S, {}^2 a}$ very nearly. And since DK is an inch and a half, we get the length in inches, counted from the apex of the cone $= \frac{1\frac{1}{2}}{S, {}^2 a}$, or $\frac{3}{2 S, {}^2 a}$. From

this we must cut off the part CD, which is $= \frac{DK}{S,\frac{1}{2}a}$, or very nearly $\frac{DK}{S,a}$, or $\frac{3}{2\,S,a}$, measured in inches, and we must make the mouth of the same width $\frac{3}{2\,S,a}$.

On the other hand, if the length of the trumpet is fixed on, we can determine the angle of the cone. For let the length (reckoned from C) be L; we have $2\,S,^2a = \frac{3}{L}$, or $S,^2a = \frac{3}{2L}$, and $S,a = \sqrt{\frac{3}{2L}}$.

Thus let 6 feet or 72 inches be chosen for the length of the cone, we have $S,a = \sqrt{\frac{3}{144}}$ $\sqrt{\frac{1}{48}}$, $= 0{,}14434$, $=$ sin. 8° 17′ for the angle of the cone; and the width at the mouth is $\frac{3}{2,S,a} = 10{,}4$ inches. This being taken from 72, leaves 61,6 inches for the length of the trumpet.

And since this trumpet confines the reflected sounds to a cone of 16° 34′, we have its magnifying power $= \frac{DT^2}{DK^2}$, $= \frac{(\frac{1}{2}DT)^2}{(\frac{1}{2}DK)^2} = \frac{S,^2 45°}{S,^2 4°\,8'\,\frac{1}{2}} = 96$ nearly. It therefore condenses the sound about 96 times; and if the distribution were uniform, it would be heard $\sqrt{96}$, or nearly 10 times farther off. For the loudness of sounds is supposed to be inversely as the square of the distance from the centre of undulation.

But before we can pronounce with precision on the performance of a speaking trumpet, we must examine into the manner in which the reflected sounds are distributed over the space in which they are all confined.

Let BKDA (Plate VI. fig. 3.) be the section of a conical trumpet by a plane through the axis: let C be the vertex of the cone, and CW its axis; let TKV be the section of a

sphere, having its centre in the vertex of the cone; and let P be a sonorous point on the surface of the sphere, and P *a f e l* the path of a line of sound lying in the plane of the section.

In the great circle of the sphere take KQ = KP, DR = DQ, and KS = KR. Draw QB *h*; also draw Q *d n* parallel to DA; and draw PB, P *d*, PA.

1. Then it is evident that all the lines drawn from P, within the cone APB, proceed without reflexion, and are diffused as if no trumpet had been used.

2. All the sonorous lines which fall from P on KB are reflected from it as if they had come from Q.

3. All the sonorous lines between BP and *d* P have suffered but one reflection; for *d n* will no more meet DAA′ so as to be reflected again.

4. All the lines which have been reflected from KB, and afterwards from DA, proceed as if they had come from R. For the lines reflected from KB proceed as if they had come from Q; and lines coming from Q and reflected by DA, proceed as if they had come from R. Therefore draw RA *o*, and also draw R *g m* parallel to KB, and draw Q *c* A *q*, Q *b g*, P *c*, and P *b*. Then,

5. All the lines between *b* P and *c* P have been twice reflected.

Again, draw SB *p*, B *r* R, *r u* Q, S *x* A, R *y x*, Q *z y*.

6. All the lines between *u* P and *z* P have suffered three reflexions.

Draw the tangents TA *t*, VB *v*, crossing the axis in W.

7. The whole sounds will be propagated within the cone *v* W *t*. For to every sonorous point in the line KD, there corresponds a point similar to Q, regulating the first reflection from KB; and a point similar to R, regulating the second reflection from DA; and a point S regulating the third reflection from KB, &c. And similar points will be found regulating the first reflection from DA, the second from KB, and the third from DA, &c.; and lines drawn

from all these through A and B must lie within the tangents TA and VB.

8. Thus the centres of reflection of all the sonorous lines which lie in planes passing through the axis, will be found in the surface of this sphere; and it may be considered as a sonorous sphere, whose sounds first concentrate in W, and are then diffused in the cone *v* W *t*.

It may be demonstrated nearly in the same manner, that the sonorous lines which proceed from P, but not in the plane passing through the axis, also proceed after various reflections, as if they had come from points in the surface of the same sphere. The only difference in the demonstration is, that the centres Q, R, S of the successive reflections are not in one plane, but in a spiral line winding round the surface of the sphere according to fixed laws. The foregoing conclusions are therefore general for all the sounds which come in all directions from every point in the area of the mouth-piece.

Thus it appears, that a conical trumpet is well fitted for increasing the force of sounds by diminishing their final divergence. For had the speaker's mouth been in the open air, the sounds which are now confined within the cone *v* W *t* would have been diffused over a hemisphere: and we see that prolonging the trumpet must confine the sounds still more, because this will make the angle BWA still smaller; a longer tube must also occasion more reflections, and consequently send more sonorous undulations to the ear at a distance placed within the cone *v* W *t*.

We have now obtained a very connected view of the whole effect of a conical trumpet. It is the same as if the whole segment TKDV were sounding, every part of it with an intensity proportional to the density of the points Q, R, S, &c. corresponding to the different points P of the mouth-piece. It is easy to see that this cannot be uniform, but must be much rarer towards the margin of the segment. It would require a good deal of discussion to show the density of these

fictitious sounding points; and we shall content ourselves with giving a very palpable view of the distribution of the sonorous rays, or the density (so to speak) of the echoes, in the different situations in which a hearer may be placed.

We may observe, in the mean time, that this substitution of a sounding sphere for the sounding mouth-piece has an exact parallel in Optics, by which it will be greatly illustrated. Suppose the cone BKDA to be a tube polished in the inside, fixed in a wall B α, perforated in BA, and that the mouth-piece DK is occupied completely by a flat flame. The effect of this on a spectator will be the same if he is properly placed in the axis, as if he were looking at a flame as big as the whole sphere. This is very evident.

It is easy to see that the line *l e* S is equal to the line *l e f a* P; therefore the reflected sounds also come to the ear in the same moments as if they had come from their respective points on the surface of the substituted sphere. Unless, therefore, this sphere be enormously large, the distinctness of articulation will not be sensibly affected, because the interval between the arrival of the different echoes of the same snap will be insensible.

Our limits oblige us to content ourselves with exhibiting this evident similarity of the progress of echo from the surface of this phonic sphere, to the progress of light from the same luminous sphere shining through a hole of which the diameter is AB. The direct investigation of the intensity of the sound in different directions and distances would take up much room, and give no clearer conception of the thing. The intensity of the sound in any point is precisely similar to the intensity of the illumination of the same point; and this is proportional to the portion of the luminous surface seen from this point through the hole directly, and to the square of the distance inversely. The intelligent reader will acquire a distinct conception of this matter from Plate VI. fig. 4. which represents the distribution of the sonorous lines,

and by consequence the degree of loudness which may be expected in the different situations of the hearer.

As we have already observed, the effect of the cone of the trumpet is perfectly analogous to the reflection of light from a polished concave, conical mirror. Such an instrument would be equally fitted for illuminating a distant object. We imagine that these would be much more powerful than the spherical or even parabolic mirrors commonly used for this purpose. These last, having the candle in the focus, also send forward a cylinder of light of equal width with the mirror. But it is well known, that oblique reflections are prodigiously more vivid than those made at greater angles. Where the inclination of the reflected light to the plane of the mirror does not exceed eight or ten degrees, it reflects about three-fourths of the light which falls on it. But when the inclination is 80, it does not reflect one-fourth part.

We may also observe, that the density of the reflected sounds by the conical trumpet ABC (Plate VI. fig. 4.) is precisely similar to that of the illumination produced by a luminous sphere TDV, shining through a hole AB. There will be a space circumscribed by the cone formed by the lines TB *t* and VA *v*, which is uniformly illuminated by the whole sphere (or rather by the segment TDV), and on each side there is a space illuminated by a part of it only, and the illumination gradually decreases towards the borders. A spectator placed much out of the axis, and looking through the hole AB, may not see the whole sphere. In like manner, he will not hear the whole sounding sphere: He may be so far from the axis as neither to see nor hear any part of it.

Assisting our imagination by this comparison, we perceive that beyond the point *w* there is no place where *all* the reflected sounds are heard. Therefore, in order to preserve the magnifying power of the trumpet at any distance, it is necessary to make the mouth as wide as the sonorous sphere. Nay, even this would be an imperfect instrument, because

its power would be confined to a very narrow space; and if it be not accurately pointed to the person listening, its power will be greatly diminished. And we may observe, by the way, that we derive from this circumstance a strong confirmation of the justness of Mr. Lambert's principles; for the effects of speaking trumpets are really observed to be limited in the way here described.—Parabolic trumpets have been made, and they fortify the sound, not only in the cylindrical space in the direction of the axis, but also on each side of it, which should not have been the case had their effect depended only on the undulations formed by the parabola in planes perpendicular to the axis. But to proceed.

Let BCA (Plate VI. fig. 5.) be the cone, ED the mouth-piece, TEDV the equivalent sonorous sphere, and TBAV the circumscribed cylinder. Then CA or CB is the length of cone that is necessary for maintaining the magnifying power at all distances. We have two conditions to be fulfilled. The diameter ED of the mouth-piece must be of a certain fixed magnitude, and the diameter AB of the outer end must be equal to that of the equivalent sonorous sphere. These conditions determine all the dimensions of the trumpet and its magnifying power. And, first, with respect to the dimensions of the trumpet.

The similarity of the triangles ECG and BCF gives $CG : ED = CF : AB$; but $CG = BF, = \frac{1}{2} AB$, and $CF = CG + GF, = GF + \frac{1}{2} AB$; therefore $\frac{1}{2} AB : ED = GF + \frac{1}{2} AB : AB$, and $AB : ED = 2GF + AB : AB$; therefore $2GF \times ED + AB \times ED = AB^2$, and $2GF \times ED = AB^2, - AB \times ED, = AB \times \overline{AB - ED}$, and $GF = \frac{AB \times \overline{AB - ED}}{2ED}$. And, on the other hand, because $AB^2 - AB \times ED = 2GF \times ED$, we have $AB^2 - AB \times ED + \frac{1}{4} ED^2 = 2GF \times ED + \frac{1}{4} ED^2$, or $\overline{AB - \frac{1}{2} ED}^2 = 2GF \times ED + \frac{1}{4} ED^2$, and $AB = \sqrt{2GF \times ED + \frac{1}{4} ED^2} + \frac{1}{2} ED$.

Let x represent the length of the trumpet, y the diameter at the great end, and m the diameter of the mouth-piece. Then $x = \frac{y \times \overline{y - m}}{2\,m}$, and $y = \sqrt{2\,x\,m + \frac{1}{4}m^2} + \frac{1}{2}m$. Thus the length and the great diameter may be had reciprocally. The useful case in practice is to find the diameter for a proposed length, which is gotten by the last equation.

Now if we take all the dimensions in inches, and fix m at an inch and a half, we have $2\,x\,m = 3\,x$, and $\frac{1}{4}\,m^2 = 0{,}5625$, and $\frac{1}{2}\,m = 0{,}75$; so that our equation becomes $y = \sqrt{3\,x + 0{,}5625} + 0{,}75$. The following table gives the dimensions of a sufficient variety of trumpets. The first column is the length of the trumpet in feet; the second column is the diameter of the mouth in inches; the third column is the number of times that it magnifies the sound; and the fourth column is the number of times that it increases the distance at which a man may be distinctly heard by its means; the fifth contains the angle of the cone.

GF feet.	AB inches.	Magnifying.	Extending.	ACB.
				° ′
1	6,8	42,6	6,5	24 53
2	9,3	77,8	8,8	18 23
3	11,2	112,4	10,6	15 18
4	12,8	146,6	12,1	13 24
5	14,2	180,4	13,4	12 04
6	15,5	214,2	14,6	11 05
7	16,6	247,7	15,7	10 18
8	17,7	281,3	16,8	9 40
9	18,8	314,6	17,7	9 08
10	19,8	347,7	18,6	8 42
11	20,7	380,9	19,5	8 18
12	21,5	414,6	20,4	7 58
15	24,	513,6	22,7	7 09
18	26,2	612,3	24,7	6 33
21	28,3	711,2	26,6	6 05
24	30,2	810,1	28,5	5 42
ED in all is = 1,5.				

The two last columns are constructed on the following considerations: We conceive the hearer placed within the cylindrical space whose diameter is BA. In this situation he receives an echo coming apparently from the whole surface TGV; and we account the effect of the trumpet as equivalent to the united voices of as many mouths as would cover this surface. Therefore the quotient obtained by dividing the surface of the hemisphere by that of the mouthpiece will express the magnifying power of the trumpet. If the chords g E, g T, be drawn, we know that the spherical surfaces, T g V, E g D, are respectively equal to the circles described with the radii T g, E g, and are therefore as T g^2 and E g^2. Therefore the audibility of the trumpet, when compared with a single voice, may be expressed by $\frac{\mathrm{T}\,g^2}{\mathrm{E}\,g^2}$. Now the ratio of T$g^2$ to Eg^2 is easily obtained. For if Ef be drawn parallel to the axis, it is plain that $\mathrm{B}f = \frac{\mathrm{BA}-\mathrm{ED}}{2}$, and that E$f$ is to f B as radius to the tangent of BCF; which angle we may call a. Therefore $\tan. a = \frac{y-m}{2x}$, and thus we obtain the angle a. But if the radius CE be accounted 1, T g is $= \sqrt{2}$, and E g is $= 2 \sin. \frac{a}{2}$. Therefore $\frac{\mathrm{T}\,g}{\mathrm{E}\,g} = \frac{\sqrt{2}}{2 \sin. \frac{a}{2}}$, and the magnifying power of the trumpet $= \frac{2}{4 \sin^2. \frac{a}{2}}, = \frac{1}{2 \sin.^2 \frac{a}{2}}$. The numbers, therefore, in the third column of the table are each $= \frac{1}{2 \sin^2. \frac{a}{2}}$.

But the more usual way of conceiving the power of the trumpet is, by considering how much farther it will enable us to hear a voice equally well. Now we suppose that the audibility of sounds varies in the inverse duplicate ratio of

the distance. Therefore if the distance d, at which a man may be distinctly heard, be increased to z, in the proportion of EG to T g, the sound will be less audible, in the proportion of T g^2 to EG^2. Therefore the trumpet will be as well heard at the distance z as the simple voice is heard at the distance d. Therefore $\frac{z}{d}$ will express the *extending power* of the trumpet, which is therefore $= \frac{\sqrt{2}}{2 \sin. \frac{}{2}}$.

In this manner were the numbers computed for the fourth column of the table.

When the angle BCA is small, which is always the case in speaking trumpets, we may, without any sensible error, consider E g as $= \frac{ED}{2}, = \frac{m}{2}$. And T g = TC $\times \sqrt{2}, =$ $\frac{AB}{2}\sqrt{2} = \frac{AB}{\sqrt{2}} = \frac{y}{\sqrt{2}}$. This gives a very easy computation of the extending and magnifying powers of the trumpet.

The extending power is $= \sqrt{2}\frac{y}{m}$.

The magnifying power is $= 2\frac{y^2}{m^2}$.

We may also easily deduce from the premises, that if the mouth-piece be an inch and a half in diameter, and the length x be measured in inches, the extending power is very nearly $= \sqrt{\frac{8}{3}x}$ and the magnifying power $= \frac{8}{3}x$.

An inconvenience still attends the trumpet of this construction. Its complete audibility is confined to the cylindrical space in the direction of the axis, and it is more faintly heard on each side of it. This obliges us to direct the trumpet very exactly to the spot where we wish it to be heard. This is confirmed by all the accounts we have of the performance of great speaking trumpets. It is evident, that by lengthening the trumpet, and therefore enlarging

its mouth, we make the lines TB *t* and VA *b* expand (Plate VI. fig. 4.): and therefore it will not be so difficult to direct the trumpet.

But even this is confined within the limits of a few degrees. Even if the trumpet were continued without end, the sounds cannot be reinforced in a wider space than the cone of the trumpet. But it is always advantageous to increase its length; for this makes the extreme tangents embrace a greater portion of the sonorous sphere, and thus increases the sound in the space where it is all reflected. And the limiting tangents TB, VA, expand still more, and thus the space of full effect is increased. But either of these augmentations is very small in comparison of the augmentation of size. If the trumpet of Plate VI. fig. 5. were made an hundred times longer, its power would not be increased one half.

We need not therefore aim at much more than to produce a cylindrical space of full effect; and this will always be done by the preceding rules, or table of constructions. We may give the trumpet a third or a fourth part more length, in order to spread a little the space of its full effect, and thereby make it more easily directed to the intended object. But in doing this we must be careful to increase the diameter of the mouth as much as we increase the length; otherwise we produce the very opposite effect, and make the trumpet greatly inferior to a shorter one, at all distances beyond a certain point. For by increasing the length while the part CG remains the same, we cause the tangents TB and VA to meet on some distant point, beyond which the sound diffuses prodigiously. The construction of a speaking trumpet is therefore a problem of some nicety; and as the trials are always made at some considerable distance, it may frequently happen that a trumpet, which is not heard at a mile's distance, may be made very audible two miles off by cutting off a piece at its wide end.

After this minute consideration of the conical trumpet, we might proceed to consider those of other forms. In particular, the hyperbolic, proposed by Cassegrain, and the parabolic, proposed by Haase, seem to merit consideration. But if we examine them merely as reflectors of echoes, we shall find them inferior to the conical.

With respect to the hyberbolic trumpet, its inaptitude is evident at first sight. For it must dissipate the echoes more than a conical trumpet. Indeed Mr. Cassegrain proceeds on quite different principles, depending on the mechanism of the aerial undulations: his aim was to increase the agitation in each pulse, so that it may make a more forcible impulse on the ear. But we are too imperfectly acquainted with this subject to decide *a priori;* and experience shows that the hyberbola is not a good form.

With respect to the parabolic trumpet, it is certain that if the mouth-piece were but a point, it would produce the most favourable reflection of all the sounds; for they would all proceed parallel to the axis. But every point of an open mouth must be considered as a centre of sound, and none of it must be kept out of the trumpet. If this be all admitted, it will be found that a conical trumpet, made by the preceding rules, will dissipate the reflected sounds much less than the parabolic.

Thus far have we proceeded on the fair consequences of the well known fact, that echoes are reflected in the same manner as light, without engaging in the intricate investigation of aerial undulations. Whoever considers the Newtonian theory of the propagation of sound with intelligence and attention, will see that it is demonstrated solely in the case of a single row of particles; and that all the general corollaries respecting the lateral diffusion of the elastic undulations are little more than sagacious guesses, every way worthy of the illustrious author, and beautifully confirmed by what we can most distinctly and accurately observe in the circular waves on the surface of still water. But they

are by no means fit for becoming the foundation of any doctrine which lays the smallest claim to the title of accurate science. We really know exceedingly little of the theory of aerial undulations; and the conformity of the phenomena of sound to these guesses of Sir Isaac Newton has always been a matter of wonder to every eminent and candid mathematician; and no other should pretend to judge of the matter. This wonder has always been acknowledged by Daniel Bernoulli; and he is the only person who has made any addition to the science of sounds that is worth mentioning. For such we must always esteem his doctrine of the secondary undulations of musical cords, and the secondary pulses of air in pipes. Nothing therefore is more unwarrantable, or more plainly shews the precipitant presumption of modern sciolists, than the familiar use of the general theory of aerial undulations in their attempts to explain the abstruse phenomena of nature (such as the communication of sensation from the organ to the sensorium by the vibrations of a nervous fluid, the reciprocal communication of the volitions from the sensorium to the muscle, nay, the whole phenomena of mind), by vibrations and vibratiunculæ.

Those who have endeavoured to improve the speaking trumpet on mechanical principles, have generally aimed at increasing the violence of the elastic undulations, that they may make a more forcible impulse on the ear. This is the object in view in the parabolic trumpet. All the undulations are converted into others which are in planes perpendicular to the axis of the instrument; so that the same little mass of air is agitated again and again in the same direction. From this it is obvious to conclude, that the total agitation will be more violent. But, in the first place, these violent agitations must diffuse themselves laterally as soon as they get out of the trumpet, and thus be weakened in a proportion that is perhaps impossible for the most expert analyst to determine. But, moreover, we are not sufficiently acquainted with the mechanism of the very first agitations, to be able

to perceive what conformation of the trumpet will cause the reflected undulations to increase the first undulations, or to check them. For it must happen, during the production of a continued sound in a trumpet, that a parcel of air, which is in a state of progressive agitation, as it makes a pulse of one sound, may be in a state of retrograde agitation, as it is part of a pulse of air producing another sound. We cannot (at least no mathematician has yet done it) discriminate, and then combine these agitations, with the intelligence and precision that are necessary for enabling us to say what is the ultimate accumulated effect. Mr. Lambert therefore did wisely in abstaining from this intricate investigation; and we are highly obliged to him for deducing such a body of demonstrable doctrine from the acknowledged, but ill understood, fact of the reflection of echoes.

We know that two sounds actually cross each other without any mutual disturbance; for we can hear either of them distinctly, provided the other is not so loud as to to stun our ears, in the same manner as the glare of the sun dazzles our eyes. We may therefore depend on all the consequences which are legitimately deduced from this fact, in the same manner as we depend on the science of catoptrics, which is all deduced from a fact perfectly similar, and as little understood.

But the preceding propositions by no means explain or comprehend all the reinforcement of sound which is really obtained by means of a speaking trumpet. In the first place, although we cannot tell in what degree the aerial undulations are increased, we cannot doubt that the reflections which are made in directions which do not greatly deviate from the axis, do really increase the agitation of the particles of air. We see a thing perfectly similar to this in the waves on water. Take a long slip of lead, about two inches broad, and having bent it into the form of a parabola, set it into a large flat trough, in which the water is about an inch deep. Let a quick succession of small drops of water

fall precisely on the focus of the parabola. We shall see the circular waves proceeding from the focus all converted into waves perpendicular to the axis; and we shall frequently see these straight waves considerably augmented in their height and force. We say generally, for we have sometimes observed that these reflected waves were not sensibly stronger than the circular or original waves. We do not exactly know to what this difference must be ascribed: we are disposed to attribute it to the frequency of the drops. This may be such, that the interval of time between each drop is precisely equal, or at least commensurable, to the time in which the waves run over their own breadth. This is a pretty experiment; and the ingenious mechanician may make others of the same kind which will greatly illustrate several difficult points in the science of sounds. We may conclude, in general, that the reflection of sounds, in a trumpet of the usual shapes, is accompanied by a real increase of the aerial agitations; and in some particular cases we find the sounds prodigiously increased. Thus when we blow through a musical trumpet, and allow the air to take that uniform undulation which can be best maintained in it, namely, that which produces its musical tone, where the whole tube contains but one or two undulations, the agitation of a particle must then be very great; and it must describe a very considerable line in its oscillations. When we suit our blast in such a manner as to continue this note, that is, this undulation, we are certain that the subsequent agitations conspire with the preceding agitation, and augment it. And accordingly we find that the sound is increased to a prodigious degree. A cor-de-chasse, or a bugle horn, when properly winded, will almost deafen the ear; and yet the exertion is a mere nothing in comparison with what we make when bellowing with all our force, but with not the tenth part of the noise. We also know that if we speak through a speaking trumpet in the key which corresponds with its dimensions, it is much more audible

than when we speak in a different pitch. These observations shew, that the loudness of a speaking trumpet arises from something more than the sole reflection of echoes considered by Mr. Lambert—the very echoes are rendered louder.

In the next place, the sounds are increased by the vibrations of the trumpet itself. The elastic matter of the trumpet is thrown into tremors by the undulations which proceed from the mouth-piece. These tremors produce pulses in the contiguous air, both in the inside of the trumpet and on that which surrounds it. These undulations within the trumpet produce original sounds, which are added to the reflected sounds: for the tremor continues for some little time, perhaps the time of three or four or more pulses. This must increase the loudness of the subsequent pulses. We cannot say to what degree, because we do not know the force of the tremor which the part of the trumpet acquires: but we know that these sounds will not be magnified by the trumpet to the same degree as if they had come from the mouth-piece: for they are reflected as if they had come from the surface of a sphere which passes through the agitated point of the trumpet, In short, they are magnified only by that part of the trumpet which lies without them. The whole sounds of this kind, therefore, proceed as if they came from a number of concentric spherical surfaces, or from a solid sphere, whose diameter is twice the length of the trumpet cone.

All these agitations arising from the tremors of the trumpet tend greatly to hurt the distinctness of articulation; because, coming from different points of a large sphere, they arrive at the ear in a sensible succession; and thus change a momentary articulation to a lengthened sound, and give the appearance of a number of voices uttering the same words in succession. It is in this way that, when we clap our hands together near a long rail, we get an echo from each post, which produces a chirping sound of some continuance. For

these reasons, it is found advantageous to check all tremors of the trumpet by wrapping it up in woollen lists. This is also necessary in the musical trumpet.

With respect to the undulations produced by the tremors of the trumpet in the air contiguous to its outside, they also hurt the articulation. At any rate, this is so much of the sonorous momentum uselessly employed; because they are diffused like common sounds, and receive no augmentation from the trumpet.

It is evident, that this instrument may be used (and accordingly was so) for aiding the hearing; for the sonorous lines are reflected in either direction. We know that all tapering cavities greatly increase external noises; and we observe the brutes prick up their ears when they want to hear uncertain or faint sounds. They turn them in such directions as are best suited for the reflection of the sound from the quarter whence the animal imagines that it comes.

Let us apply Mr. Lambert's principle to this very interesting case, and examine whether it be possible to assist dull hearing in like manner as the optician has assisted imperfect sight.

The subject is greatly simplified by the circumstances of the case; for the sounds to which we listen generally come in nearly one direction, and all that we have to do is to produce a constipation of them. And we may conclude, that the audibility will be proportional to this constipation.

Therefore let ACB, Plate VI. fig. 6. be the cone, and CD its axis. The sound may be conceived as coming in the direction RA, parallel to the axis, and to be reflected in the points A, *b*, *c*, *d*, *e*, till the angle of incidence increases to 90°; after which the subsequent reflections send the sound out again. We must therefore cut off a part of the cone; and, because the lines increase their angle of incidence at each reflection, it will be proper to make the angle of the cone an aliquot part of 90°, that the least incidence may amount

precisely to that quantity. What part of the cone should be cut off may be determined by the former principles. Call the angle ACD, a. We have C $e = \frac{\text{CA} \cdot \text{sin. } a}{\text{sin.}(2n+1)a}$, when the sound gets the last useful reflection. Then we have the diameter of the mouth AB $= 2$ CA $\cdot$ sin. a, and that of the other end $ef =$ C $e \cdot 2$ sin. a. Therefore the sounds will be constipated in the ratio of CA^2 to C e^2, and the trumpet will bring the speaker nearer in the ratio of CA to C e.

When the lines of reflected sound are thus brought together, they may be received into a small pipe perfectly cylindrical, which may be inserted into the external ear. This will not change their angles of inclination to the axis nor their density. It may be convenient to make the internal diameter of this pipe $\frac{1}{3}$ of an inch. Therefore C $e \cdot$ sin. a is $= \frac{1}{6}$ of an inch. This circumstance, in conjunction with the magnifying power proposed, determines the other dimensions of the hearing trumpet. For C $e = \frac{1}{6 \text{ sin. } a} = \frac{\text{CA sin. } a}{\text{sin. } (2n+1)a}$, and CA $= \frac{\text{sin. } (2n+1)a}{6 \text{ sin.}^2 a}$.

Thus the relation of the angle of the cone and the length of the instrument is ascertained, and the sound is brought nearer in the ratio of CA to C e, or of sin. $(2n+1)a$ to sin. a. And seeing that we found it proper to make $(2n+1)a = 90°$, we obtain this very simple analogy. 1 : sin. $a =$ CA : C e. And the sine of $\frac{1}{2}$ the angle of the cone is to radius as 1 to the approximating power of the instrument.

Thus let it be required that the sound may be as audible as if the voice were twelve times nearer. This gives $\frac{\text{CA}}{Ce} = 12$. This gives sin. $a = \frac{1}{12}$, and $a = 4° 47'$, and the angle of the cone $= 9° 34'$. Then CA $= \frac{1}{6 \text{ sin. }^2 a} = \frac{1}{6 \times \frac{1}{144}} = \frac{144}{6}$,

$=24$. Therefore the length of the cone is 24 inches. From this take $C\ e = \frac{CA}{12} = 2$, and the length of the trumpet is 22 inches. The diameter at the mouth is $2\ C\ e$, $= 4$ inches. With this instrument one voice should be as loud as 144.

If it were required to approximate the sound only four times, making it 16 times stronger than the natural voice at the same distance, the angle ACB must be 29°; A e must be 2 inches, AB must be $1\frac{1}{3}$d inches, and $e\ f$ must be $\frac{1}{3}$d of an inch.

It is easy to see, that when the size of the ear-end is the same in all, the diameters at the outer end are proportional to the approximating powers, and the length of the cones are proportional to the magnifying powers.

We shall find the parabolic conoid the preferable shape for an acoustic trumpet; because the sounds come into the instrument in a direction parallel to the axis, they are reflected so as to pass through the focus. The parabolic conoid must therefore be cut off through the focus, that the sounds may not go out again by the subsequent reflections; and they must be received into a cylindrical pipe of $\frac{1}{3}$d of an inch in diameter. Therefore the parameter of this parabola is $\frac{1}{3}$d of an inch, and the focus is $\frac{1}{12}$th of an inch from the vertex. This determines the whole instrument; for they are all portions of one parabolic conoid. Suppose that the instrument is required to approximate the sound 12 times, as in the example of the conical instrument. The ordinate at the mouth must be 12 times the 6th of an inch, or 2 inches; and the mouth diameter is 4 inches, as in the conical instrument. Then, for the length, observe, that DC in Plate VI. Fig. 7. is $\frac{1}{6}$th of an inch, and MP is 2 inches, and AC is $\frac{1}{576}$th of an inch and $DC^2 : MP\frac{1}{2} = AC : AP$. This will give AP $= 12$ inches, and CP $= 11\frac{11}{12}$ths; whereas in the conical tube it was 22. In like manner an instrument which approximates the sounds 4 times, is only $1\frac{1}{4}$th inches long, and

$1\frac{1}{4}$th inches diameter at the big end. Such small instruments may be very exactly made in the parabolic form, and are certainly preferable to the conical. But since even these are of a very moderate size when intended to approximate the sound only a few times, and as they can be accurately made by any tin-man, they may be of more general use. One of 12 inches long, and 3 inches wide at the big end, should approximate the sound at least 9 times.

A general rule for making them.—Let m express the approximating power intended for the instrument. The length of the instrument in inches is $\frac{m \times \overline{m-1}}{6}$, and the diameter at the mouth is $\frac{m}{3}$. The diameter at the small end is always $\frac{1}{3}$ of an inch.

In trumpets for assisting the hearing, all reverberation of the trumpet must be avoided. It must be made thick, of the least elastic materials, and covered with cloth externally. For all reverberation lasts for a short time, and produces new sounds which mix with those that are coming in.

We must also observe, that no acoustic trumpet can separate those sounds to which we listen from others that are made in the same direction. All are received by it, and magnified in the same proportion. This is frequently a very great inconvenience.

There is also another imperfection, which we imagine cannot be removed, namely, an odd confusion, which cannot be called indistinctness, but a feeling as if we were in the midst of an echoing room. The cause seems to be this: Hearing gives us some perception of the direction o the sounding object, not indeed very precise, but sufficiently so for most purposes. In all instruments which we have described for constipating sounds, the last reflections are made in directions very much inclined to the axis, and inclined in many different degrees. Therefore they have the appearance of coming from different quarters; and instead of the perception of a single speaker, we have that of a sounding

surface of great extent. We do not know any method of preventing this, and at the same time increasing the sound.

There is an observation which it is of importance to make on this theory of acoustic instruments. Their performance does not seem to correspond to the computations founded on the theory. When they are tried, we cannot think that they magnify so much: Indeed it is not easy to find a measure by which we can estimate the degrees of audibility. When a man speaks to us at the distance of a yard, and then at the distance of two yards, we can hardly think that there is any difference in the loudness; though theory says, that it is four times less in the last of the two experiments; and we cannot but adhere to the theory in this very simple case, and must attribute the difference to the impossibility of measuring the loudness of sounds with precision. And because we are familiarly acquainted with the sound, we can no more think it four times less at twice the distance, than we can think the visible appearance of a man four times less when he is at a quadruple distance. Yet we can completely convince ourselves of this, by observing that he covers the appearance of four men at that distance. We cannot easily make the same experiment with voices.

But, besides this, we have compared two hearing trumpets, one of which should have made a sound as audible at the distance of 40 feet as the other did at 10 feet distance; but we thought them equal at the distance of 40 and 18. The result was the same in many trials made by different persons, and in different circumstances. This leads us to suspect some mistake in Mr. Lambert's principles of calculation: and we think him mistaken in the manner of estimating the intensity of the reflected sounds. He conceives the proportion of intensity of the simple voice and of the trumpet to be the same with that of the surface of the mouth-piece to the surface of the sonorous hemisphere, which he has so ingeniously substituted for the trumpet. But this seems to suppose, that the whole surface, generated by the revolution of the quad-

rantal arch TEG round the axis CG (Plate VI. fig. 4.), is equally sonorous. We are assured that it is not; For even if we should suppose that each of the points Q, R, and S (Plate VI. fig. 3.), are equally sonorous with the point P, these points of reflection do not stand so dense on the surface of the sphere as on the surface of the mouth-piece. Suppose them arranged at equal distances all over the mouth-piece, they will be at equal distances also on the sphere, only in the direction of the arches of great circles which pass through the centre of the mouth-piece. But in the direction perpendicular to this, in the circumference of small circles having the centre of the mouth-piece for their pole, they must be rarer in the proportion of the sine of their distance from this pole. This is certainly the case with respect to all such sounds as have been reflected in the planes which pass through the axis of the trumpet; and we do not see (for we have not examined this point) that any compensation is made by the reflexion which is not in planes passing through the axis. We therefore imagine, that the trumpet does not increase the sound in the proportion of $g\ E^2$ to $g\ T^2$ (Plate VI. fig. 5.), but in that of $\frac{g\ E^2}{GE}$ to $\frac{g\ T^2}{CT}$.

Mr. Lambert seems aware of some error in his calculation, and proposes another, which leads nearly to this conclusion, but founded on a principle which we do not think in the least applicable to the case of sounds.

MARINE TRUMPET.

THIS is a stringed instrument, invented in the 16th century by an Italian artist Marino or Marigni, and called a *trumpet*, because it takes only the notes of the trumpet, with all its omissions and imperfections, and can therefore execute only such melodies as are fitted for that instrument. It is a very curious instrument, though of small musical powers, because its mode of performance is totally unlike that of other stringed instruments; and it deserves our very particular attention, because it lays open the mechanism of musical sounds more than any thing we are acquainted with; and we shall therefore make use of it in order to communicate to our readers a philosophical theory of music, which we have already treated in detail as a liberal or scientific art

The trumpet marine is commonly made in the form of a long triangular pyramid, ABCD, (Plate VI. fig. 8.) on which a single string EFG is strained over a bridge F by means of the finger pin L. At the narrow end are several frets 1, 2, 3, 4, 5, &c. between E and K, which divide the length EF,

into aliquot parts. Thus E 1 is $\frac{1}{32}$ of EF, E 2 is $\frac{1}{31}$, and so on. The bow is drawn lightly across the cord at H, and the string is stopped by pressing it with the finger immediately above the frets, but not so hard as to make it touch the fret. When the open string is sounded, it gives the fundamental note. If it be stopped, in the way now described, at $\frac{1}{3}$d of its length from E, it yields the 12th of the fundamental; if stopped at $\frac{1}{4}$th, it gives the double octave; if at $\frac{1}{5}$th, it gives the 17th major, &c. In short, it always gives the note corresponding to the length of the part between the fret and the note E. The sounds resemble those of a pipe, and are indeed the same with those known by the name *harmonics*, and now executed by every performer on instruments of the viol or violin species. But in order to increase the noise, the bridge F is constructed in a very particular manner. It does not rest on the sound-board of the instrument through its whole breadth, but only at the corner *a*, where it is firmly fixed. The other extremity is detached about $\frac{1}{100}$th of an inch from the sound-board; and thus the bridge, being made to tremble by the strong vibration of the thick cord, rattles on the sound-board, or on a bit of ivory glued to it. The usual way in which this motion is procured, is to have another string passing under the middle of the bridge in such a manner that, by straining it tight, we raise the corner *b* from the sound-board to the proper height. This contrivance increases prodigiously the noise of the instrument, and gives it somewhat of the smart sound of the trumpet, though very harsh and coarse. But it merits the attention of every person who wishes to know any thing of the philosophy of musical sounds, and we shall therefore say as much on the subject as will conduce to this effect.

Galileo, as we have observed in the article TEMPERAMENT, was the first who discovered the real connection between mathematics and music, by demonstrating that the times of the vibrations of elastic cords of the same matter and size,

and stretched by equal weights, are proportional to the lengths of the strings. He inferred from this that the musical pitch of the sound produced by a stretched cord depended solely on the frequency of the vibrations. Moreover, not being able to discover any other circumstance in which those sounds physically resembled each other, and reflecting that all sounds are immediately produced by agitations of air acting on the ear, he concluded that each vibration of the cord produced a sonorous pulse in the air, and therefore that the pitch of *any* sound whatever depended on the frequency of the aerial pulses. In this way alone the sound of a string, of a bell, of an organ pipe, and the bellow of a bull, may have the same pitch. He could not, however, demonstrate this in any case but the one above mentioned. But he was encouraged to hope that mathematicians would be able to demonstrate it in all cases, by his having observed that the same proportions obtained in organ pipes as in strings stretched by equal weights. But it required a great progress in mechanical philosophy, from the state in which Galileo found it, before men could speculate and reason concerning the pulses of air, and discover any analogy between them and the vibrations of a string. This analogy, however, was discovered, and its demonstration completed, as we shall see by and by. In the mean time, Galileo's demonstration of the vibrations of elastic cords became the foundation of all musical philosophy. It must be thoroughly understood before we can explain the performance of the trumpet marine.

The demonstration of Galileo is remarkable for that beautiful simplicity and perspicuity which distinguish all the writings of that great mechanician, and it is the elementary proposition in all mechanical treatises of music. Few of them indeed contain any thing more; but it is extremely imperfect, and is just only on the supposition that all the matter of the string is collected at its middle point, and that the rest of it has elasticity without *inertia*. This did not suit the

accurate knowledge of the 17th century, after Huygbens and Newton had given the world a taste of what might be done by prosecuting the Galilean mechanics. When a musical cord has its middle point drawn aside, and it is strained into the shape of two strait lines, if it be let go, it will be observed not to vibrate in this form. It may easily be seen in the extremity of its excursions, where it rests, before it returns by its elasticity. The reason is this (see Plate VI. fig. 9.) When the middle point C of the cord is drawn aside, and the cord has the form of two straight lines AC, CB, this point C, being pulled in the directions CA, CB, at once, is really accelerated in the direction CD, which bisects the angle ACB; and if it were then detached from the rest of the material cord, it would move in that direction. But any other point *f* between C and B has no accelerating force whatever acting on it. It is equally pulled in the directions *f* C and *f* B. The particle C therefore is obliged to drag along with it the inert matter of the rest of the cord; and when it has come to any intermediate situation *c*, the cord cannot have the form of two straight lines A *c*, *c* B, with the particle *f* situated in *f*. This particle will be left somewhat behind, as in φ, and the cord will have a curved form A *c* φ B; and in this form it will vibrate, going to the other side, and assuming, not the rectilineal form ADB, but the curved form A δ B. That every particle of the curve A *c c'f* B is now accelerated toward the axis AB is evident, because every part is curved, and the whole is strained toward A and B, which tends to straiten every part of it. But in order that the whole may arrive at the axis in one moment, and constitute a straight line AB, it is evidently necessary that the accelerating force on every particle be as the distance of the particle from that point of the axis at which it arrives. It is well known to the mathematician that the accelerating force by which any particle is urged towards a rectilineal position, with respect to the adjoining particles, is proportional to the curvature. Our readers, who are not

familiar with such discussions, may see the truth of this fundamental proposition by considering the whole of A *c* B as only a particle or minute portion of a curve, magnified by a microscope. The force which strains the curve may be represented by *c* A or AE. Now it is well known (and is the foundation of Galileo's demonstration) that the straining force is to the force with which *c* is accelerated in the direction *c* E as A *c* to *c* D, or as AE to *c* D, or as AE to twice *c* E. Now *c* E is the measure of the curvature of A *c* B, being its deflection from a right line. Therefore when the straining force is the same all over the curve, the accelerating force, by which any portion of it tends to become straight, is proportional to the curvature of that portion. And if *r* be the radius of a circle passing through A, *c*, and B, and coinciding with this element of a curve, it is plain that $c\,D : c\,A = c\,A : r$, or that the radius of curvature is to the element *c* A as the extending force to the accelerating force; and $c\,D = \frac{c\,A^2}{r}$; and is inversely as *r*, or directly as the curvature.

Hence we see the nature of that curve which a musical cord must have, in order that all its parts may arrive at the axis at once. The curvature at *c* must be to the curvature at *f* as E *c* to *g f*. But this may not be enough. It is farther necessary that when *c* has got half way to E, the curvature in the different points of the new curve into which the cord has now arranged itself, be also, in every point, proportional to the distance from the axis. Now this *will* be the case if the extreme curve has been such. For, taking the cord in any other successive shape, the distance which each point has gone in the same moment must be proportional to the force which impelled it; therefore the remaining distances of all the points from the axis will have the same proportions as before. And the geometrical and evident consequence of this is, that the curvatures will also be in the same proportion.

Therefore a cord that is once arranged in this form will always preserve it, and will vibrate like a cycloidal pendulum, performing its oscillations in equal times, whether they be wide or narrow. Therefore since this perfect isochronism of vibrations is all that is wanted for preserving the same musical pitch or tone, this cord will always have the same note.

This proposition was the discovery of Dr. Brooke Taylor, one of the ornaments of our country, and is published in his celebrated work *Methodus Incrementorum*. The investigation, however, and the demonstration in that work, are so obscure and so tedious, that few had patience to peruse them. It was more elegantly treated afterwards by the Bernoullis and others. The curve got the name of the *Taylorean curve;* and is considered by many eminent mathematicians as a trochoid, *viz.* the curve described by a point in the nave or spoke of a wheel while the wheel rolls along a straight line. But this is a mistake, although it is allied to the trochoid in the same manner that the figure of sines is allied to the cycloid. Its physical property intitles it to the name of the HARMONICAL CURVE. As this curve is not only the foundation of all our knowledge of the vibration of elastic cords, but also furnishes an equation which will lead the mathematician through the whole labyrinth of aerial undulations, and be of use on many other occasions; and as the first mathematicians have, through inattention, or through enmity to Dr. Taylor, affected to consider it as the trochoid already well known to themselves—we shall give a short account of its construction and chief properties, simplified from the elegant description given by Dr. Smith in his Harmonics.

Let SDTV, QERP (Plate VI. fig. 10.), be circles described round the centre C. Draw the diameters QCR, ECP, cutting each other at right angles. From any point G in the exterior circle draw the radius GC, cutting the interior circle in F, draw KHFI parallel to QCR, and make HI,

HK, each equal to the arch EG. Let this be done for every point of the quadrantal arch EGR. The points I, K, are in the harmonic curve; that is, the curve AKDIB passing through the points K and I, determined by this construction, has its curvature in every point K proportional to the distance KN from the base AB.

To demonstrate this, draw FL perpendicular to the axis, and join EL. Take another point g in the outer circle indefinitely near to G. Draw $g\ c$, cutting the inner circle in f, and $f\ h$ and $f\ l$ perpendicular to DC, CT, and join E l. Then suppose two lines K m $k\ m$ perpendicular to the curve in K and k. They must meet in m, the centre of the equicurve circle. Draw KN n perpendicular to the base, and $m\ n$ parallel to it, and join $k\ n$. Lastly, draw XL x perpendicular to EL.

It is plain that k O, the difference of HK and $h\ k$, is equal to G g, the difference of GE and g E, and that KO is equal to F r, and L l to $r\ f$. Also, because ELX is a right angle, $\mathrm{EX} = \frac{\mathrm{EL}^2}{\mathrm{EC}}$.

We have F r : F f = CL : CF, = CL : CD.
F f : G g = CD : CE.
Therefore F r : G g, or KO : Ok = CL : CE.

The triangles ECL and k OK are therefore similar, as are also k OK and K $n\ m$, and consequently ECL and K $n\ m$; and because EC is parallel to K n, EL is parallel to K m. For the same reason $k\ m$ is parallel to El, and the triangles E $l\ x$ and m K k are similar, and

L x : K k = LE : K m
and L x : K k = EC : K n. But farther,
L x : L l = CE : EL
L l : F f = KN : CD, being = FL : FC
F f : G g = CD : CE, being = F f : k O
G g : K k = CE : EL, being = kO : K k.
Therefore L x : K k = KN × CE : EL^2, = KN : EX.

Therefore $KN : EX = LE : K\,m$, and $K\,m = \frac{EX \cdot LE}{KN}$, and $KN : EX = CE : K\,n$, and $K\,n = \frac{EX \cdot CE}{KN}$.

In the very narrow vibrations of musical cords, CD is exceedingly small in comparison with CE, so that EX·EL, or EX·CE, may, without sensible error, be taken for CE^2, and then we obtain K m or K n (which hardly differ) $= \frac{CE^2}{KN}$, and therefore the curvature is proportional to KN. The small deviation from this ratio would seem to shew that this construction does not give the harmonic curve with accuracy. But it is not so. For it will be found, that although the curvature is not as KN, it is still proportional to the space which any particle K must really describe in order to arrive at the axis. These paths are lines whose curvatures diminish as they approach to DC.

We see 1*st*, that the base ACB of the curve is equal to the semicircular arch QER.

2*d*, Also that the tangent KZ in any point K is perpendicular to EL.

3*d*, We learn that the curvature at A and B is nothing, for in these two points KN is nothing.

4*th*, The radius of curvature at D is precisely $= \frac{CE^2}{CD}$.

Therefore, as the string approaches the axis, and CD diminishes, the curvature diminishes in the same proportion. The vibrations therefore are performed like those of a pendulum in a cycloid, and are isochronous, whether wide or narrow, and therefore the musical pitch is constant.

This is not strictly true, because in the wide vibrations the extension or extending force is somewhat greater. Hence it is that a string when violently twanged sounds a little sharper at the beginning. Dr. Long made a harpsichord whose strings were stretched by weights, by which this imperfection was removed.

It is proper to exhibit the curvature at D in terms of the

length AB, and of the greatest excursion CD. Therefore let c be the circumference of a circle whose diameter is 1. Let AB the length of the cord be = L, and let CD the $\frac{1}{2}$ breadth of the vibration be B.

We had a little ago $Dm = \frac{CE^2}{CD}$, but $c : 1 = AB : CE$, and $CE = \frac{AB}{c}$, and $CE^2 = \frac{AB^2}{c^2}$. Therefore $Dm = \frac{AB^2}{c^2 \times CD}$, $= \frac{L^2}{9{,}87CD}$ nearly.

We can now tell the number of vibrations made in a second by a string. This we obtain by comparing its motion, when impelled by the accelerating force which acts on it, with its motion when acted on by its weight only. Therefore let L be the length of a string, and W its weight, and let E be the straining weight, or extending force. Let f be the force which accelerates the particle D d of the cord, and w the weight of that particle, while W is the weight of the whole cord. Let z be the space which the particle D d would describe during the time of one vibration by the uniform action of the force f, and let S be the space which it would describe in the same time by its weight w alone. Then (DYNAMICS, No. 103. cor. 6.) the time in which f would impel the particle D d along $\frac{1}{2}$ DC, is to the time of one vibration as $1 : c$. And $\frac{1}{2}$ DC is to z as the square of the time of describing $\frac{1}{2}$ DC is to the square of the time of describing z; that is, $1 : c^2 = \frac{1}{2}$ DC : z, and c^2 DC $= 2z$.

Now, by the property of the harmonic curve,

$$AB : D\,m = 2z : AB$$
$$\text{But } D\,m : D\,d = E : f$$
$$\text{And } D\,d : AB = w : W$$
$$\text{Therefore } 2z \cdot E \cdot w = AB \cdot f \cdot W$$
$$\text{And } f : w = 2z \times E : AB \times W$$
$$\text{But } w : f = 2S : 2z$$
$$\text{Therefore } 2S \times E = AB \times W$$
$$\text{And } 2E : W = AB : S.$$

That is, a musical chord, extended by a force E, performs one vibration DCV in the time that a heavy body describes a space S, which is to the length of the cord as its weight is to twice the extending force.

Now let g be the space through which a heavy body falls in one second, and let the time of vibration (estimated in parts of a second) be T. We have

$$AB : S = 2E : W$$
$$S : g = T^2 : 1^2$$
$$\text{Therefore } AB : g = 2E \cdot T^2 : W$$
$$\text{And } AB \times W = T^2 \times 2E \times g$$
$$\text{Therefore } T^2 = \frac{AB \times W}{2g.E}, \text{ and } T = \sqrt{\frac{AB \times W}{2g.E}}.$$

Let n be the number of vibrations made in a second.

$$n = \frac{1}{T} = \sqrt{\frac{2g.E}{AB.W}} = \sqrt{\frac{2gE}{L.W}}.$$

If the length of the cord be measured in feet, $2g$ is very nearly 32. If in inches, $2g$ is 386, more nearly. Therefore $n = \sqrt{\frac{32E}{L.W}}$ or $\sqrt{\frac{386E}{L.W}}$. This may easily be compared with observation. Dr. Smith hung a weight of 7 pounds, or 49,000 grains, on a brass wire suspended from a finger pin, and shortened it till it was in perfect unison with the double octave below the open string D of a violin. In this state the wire was 35,55 inches long, and it weighed 31 grains.

Now $\sqrt{\frac{384 \times 49000}{35,55 \times 31}} = 130,7 = n$. This wire, therefore, ought to make 130,7 vibrations in a second. Dr. Smith proceeded to ascertain the number of aerial pulses made by this sound, availing himself of the theory of the beats of tempered consonances invented by himself. On his fine chamber organ he tuned upwards the perfect fifths DA, A e, e b, and then tuned downward the perfect 6th e d. Thus he obtained an octave to D, which was too sharp by a

comma, and he found that it beat 65 times in 20 seconds. Therefore the number of vibrations was $\frac{65}{20}$ 81, or 263,25. These were complete pulses or motions from D to V and back again, and therefore contained $526\frac{1}{2}$ such vibrations as we have now been considering. The double octave below should make $\frac{1}{4}$th of this, or 131,6, which is not a complete vibration more than the above theory requires: more accurate coincidence is needless *.

This theory is therefore very completely established, and it may be considered as one of the finest mechanical problems which has been solved in the last century. We mention it with greater minuteness, because the merit of Dr. Taylor is not sufficiently attended to. Mr. Rameau, and the other great theorists in music, make no mention of him; and such as have occasion to speak of the absolute number of vibrations made by any musical note, always quote Mr. Sauveur of the French academy. This gentleman has written some very excellent dissertations on the theory of music, and Sir Isaac Newton in his *Principia* often quotes his authority. He has given the actual determination of the number of vibrations of the note C, obtained in a manner similar to that practised by Dr. Smith on his chamber organ, and which agrees extremely well with that measure. But Mr. Sauveur has also given a mechanical investigation of the problem, which gives the same number of vibrations that he observed. We presume that Rameau and others took the demonstration for good; and thus Mr. Sauveur passes on the Continent for the discoverer of this theorem. But it was not published till 1716, though read in 1713; whereas Dr. Taylor's demonstration was read to the Royal Society in May 1714. But this demonstration of Mr. Sauveur is a mere paralogism, where errors compensate errors; and the assumption on which he proceeds is quite gratuitous, and has nothing to

* The coincidence will appear still more accurate if the proper number 386 be used instead of $384 = 12 \times 32$ in the calculation.

do with the subject. Yet John Bernoulli, from enmity to Taylor and the English mathematicians, takes not the least notice of this sophisticated demonstration, accommodated to the experiment, and so devoid of any pretensions to argument, that this severe critic could not but see its falsity.

Sauveur was one of the first who observed distinctly that remarkable fact which Mr. Rameau made the foundation of his musical theory, *viz.* that a full musical note is accompanied by its octave, its twelfth, and its seventeenth major. It had been casually observed before, by Marsennus, by Perrault, and others; but Sauveur tells distinctly how to make the observation, and affirms it to be true in all deep notes. Rameau asserts it to be universally and necessarily true in all notes, and the foundation of all musical pleasure.

It had been discovered before this time, that not only a full note caused its unison to resound, but also that a 12th, being sounded near any open string, the string *resounded* to this 12th. It does the same to a 15th, a 17th major, a 22d, &c.

Dr. Wallis added a very curious circumstance to this observation. Two of his pupils, Mr. Noble and Mr. Pigott, in 1673, amusing themselves with these resonances, observed, that if a small bit of paper be laid on the string of a violin which is made to resound to its unison, the paper is thrown off: a proof that the string resounded by really vibrating, and that it is thrown into these vibrations by the pulses of the air produced by the other string. In like manner the paper is thrown off when the string resounds to its octave. But the young gentlemen observed, that when the paper was laid on the middle point of the string, it remained without agitation, although the string still resounded. They found the same thing when they made the string resound to its 12th: papers laid on the two points of division lay still, but were thrown off when laid on any other place. In short,

they found it a general rule, that papers laid on any points of division corresponding to the note which was resounded, were not agitated.

Dr. Wallis (the greatest theorist in music of the 17th century) justly concluded that these points of the resounding string were at rest, and that the intermediate parts were vibrating, and producing the notes corresponding to their lengths.

From this Mr. Sauveur, with great propriety, deduced the theory of the performance of the trumpet marine, the vielle, the clavichord, and some other instruments.

When the string of the trumpet marine is gently stopped at $\frac{1}{2}$, and the bow drawn lightly across it at H (Plate VI. fig. 8), the full vibration at the finger is stopped, but the string is thrown into vibrations of some kind, which will either be destroyed or may go on. It is of importance to see what circumstance will permit their continuance.

Suppose an elastic cord put into the situation ABCDE (Plate VI. fig. 11.) such that AB, BC, CD, DE, are all equal, and that BCD is a straight line. Let the point C be made fast, and the two points B and D be let go at once. It is evident that the two parts will immediately vibrate in two harmonical curves ABC and CDE, which will change to A *b* C and C *d* E, and so on alternately. It is also evident that if a line FCG be drawn touching the curve ABC, it will also touch the curve CDE; and the line which touches the curve A *b* C in C, will also touch the curve C *d* E. In every instant the two halves of the cord will be curves which have a common tangent in the point C. The undoubted consequence of this is, that the point C will not be affected by these vibrations, and its fixture may be taken away. The cord will continue to vibrate, and will give the sound of the octave to its fundamental note.

The condition, then, which must be implemented, in order that a string may resound to its octave, or take the sound of its octave, is simply this, that its two parts may vibrate

equally in opposite directions. This is evidently possible; and when the bow is drawn across the string of the trumpet marine at H, and irregular vibrations are produced in the whole string, those which happen to be in one direction on both sides of the middle point, where it is gently stopped by the finger, will destroy each other, and the conspiring ones will be instantly produced, and then every succeeding action of the bow will increase them.

The same thing must happen if a string is gently stopped at one-third of its length; for there will be the same equilibrium of forces at the two points of division, so that the fixtures of these points may be removed, and the string will vibrate in three parts, sounding the 12th of the fundamental.

We may observe, by the way, that if the bow be drawn across the string at one of the points of division, corresponding to the stopping at the other end of the string, it will hardly give any distinct note. It rattles, and is intolerably harsh. The reason is plain: The bow takes some hold of the point C, and drags it along with it. The cord on each side of C is left behind, and therefore the two curves cannot have a common tangent at C. The vibrations into which it is thus jogged by the bow destroy each other.

We now see why the trumpet marine will not sound every note. It will sound none but such as correspond to a division of the string into a number of equal parts, and its note will be in unison with a string equal to one of those parts. Therefore it will *first* of all sound the fundamental, by its whole length;

2. Its octave, corresponding to - $\frac{1}{2}$ its length.
3. The 12th, - - - - $\frac{1}{3}$
4. The 15th, or double octave, - $\frac{1}{4}$
5. The 17th, - - - $\frac{1}{5}$
6. The 19th, - - - - $\frac{1}{6}$
7. The 21st, which is not in the diatonic scale of our music, - $\frac{1}{7}$
8. The triple octave, or 22d - $\frac{1}{8}$

9. The 23d, or 2d in the scale of the triple octave, - - $\frac{1}{9}$ its length
10. The 24th, or 3d in this scale, $\frac{1}{10}$
11. The 25th, a false 4th of this scale, $\frac{1}{11}$
12. The 26th, a perfect 5th of this scale, $\frac{1}{12}$
13. The 27th, a false 6th of ditto, $\frac{1}{13} = \frac{3}{39}$ or $\frac{3}{40}$
14. The 28th, a false 7th minor, $\frac{1}{14}$
15. The 28th, a perfect 7th major, $\frac{1}{15}$
16. The quadruple octave, - $\frac{1}{16}$

Thus we see that this instrument will not execute all music, and indeed will not complete any octave, because it will neither give a perfect 4th nor 6th. We shall presently see that these are the very defects of the trumpet.

This singular stringed instrument has been described in this detail, chiefly with the view of preparing us for understanding the real trumpet. The VIELLE, SAVOYARDE, or HURDYGURDY, performs in the same manner. While the wheel rubs one part of the string like a bow, the keys gently press the strings, in points of aliquot division, and produce the harmonic notes.

It is to prevent such notes that the part of harpsichord wires, lying between the bridge and the pins, are wrapped round with list. These notes would frequently disturb the music.

Lastly on this head, the Æolian harp derives its vast variety of fine sounds from this mode of vibration. Seldom do the chords perform their fundamental or simple vibrations. They are generally sounding some of the harmonies of their fundamentals, and give us all this variety from strings tuned in unison.

MUSICAL TRUMPET.

The Musical Trumpet is a wind instrument which sounds by pressing the closed lips to the small end, and forcing the wind through a very narrow aperture between the lips. This is one of the most ancient of musical instruments, and has appeared in all nations in a vast variety of forms. The conch of the savage, the horn of the cow-herd and of the postman, the bugle horn, the lituus and tuba of the Romans, the military trumpet, and the trombone, the *cor de chasse* or French horn—are all instruments winded in the same manner, producing their variety of tones by varying the manner and force of blowing. The serpent is another instrument of the same kind, but producing part of its notes by means of holes in the sides.

Although the trumpet is the simplest of all musical instruments, being nothing but a long tube, narrow at one end and wide at the other, it is the most difficult to be explained. To understand how sonorous and regulated undulations can be excited in a tube without any previous vibration of reeds to form the waves at the entry, or of holes to vary the notes, requires a very nice attention to the mechanism of aerial undulations, and we are by no means certain that we have as yet hit on the true explanation. We are certain, however, that these aerial undulations do not differ from those produced by the vibration of strings; for they make strings resound in the same manner as vibrating cords do. Galileo, however, did not know this argument

for his assertion that the musical pitch of a pipe, like that of a cord, depended on the frequency alone of the aerial undulations; but he thought it highly probable, from his observations on the structure of organs, that the notes of pipes were related to their lengths in the same manner as those of wires, and he expressly makes this remark. Newton, having discovered that sound moved at the rate of about 960 feet per second, observed that, according to the experiments of Mr. Sauveur, the length of an open pipe is half the length of an aerial pulse. This he could easily ascertain by dividing the space described by sound in a second by the number of pulses.

Daniel Bernoulli, the celebrated promoter of the Newtonian mechanics, discovered, or at least was the first who attentively marked, some other circumstances of resemblance between the undulations of the air in pipes and the vibrations of wires. As a wire can be made, not only to vibrate in its full length, sounding its fundamental note, but can also be made to subdivide itself, and vibrate like a portion of the whole, with points of rest between the vibrating portions, when it gives one of its harmonic notes; so a pipe can not only have such undulations of air going on within it as are competent to the production of its fundamental note, but also those which produce one of its harmonic notes. Every one knows that when we force a flute, by blowing too strongly, it quits its proper note, and gives the octave above. Forcing still more, produces the 12th. Then we can produce the double octave or 15th, and the 17th major, &c. In short, by attending to several circumstances in the manner of blowing, all the notes may be produced from one very long pipe that we produce from the trumpet marine, and in precisely the same order, and with the same omissions and imperfections. This alone is almost equivalent to a proof that the mechanism of the undulations of air in a pipe is analogous to that of the vibrations of an elastic cord. Having with so great success investigated the mechanism of the partial vibrations of wires, and also another

kind of vibrations which we shall mention afterwards, incomparably more curious and more important in the philosophy of musical sounds, Mr. Bernoulli undertook the investigation of those more mysterious motions of air which are produced in pipes; and in a very ingenious dissertation, published in the memoirs of the Academy of Paris for 1762, &c. he gives a theory of them, which tallies in a wonderful manner with the chief phenomena which we observe in the wind instruments of the flute and trumpet kind. We are not, however, so well satisfied with the truth of his *assumptions* respecting the state of the air, and the precise form of the undulations which he assigns to it; but we see that, notwithstanding a probability of his being mistaken in these circumstances (it is with great deference that we presume to suppose him mistaken), the chief propositions are still true; and that the changes from note to note must be produced in the order, though perhaps not in the precise manner, assigned by him.

It is by no means easy to conceive, with clearness, the way in which musical undulations are excited in the various kinds of trumpets. Many who have reputation as mechanicians, suppose that it is by means of vibrations of the lips, in the same manner as in the hautboy, clarionette, and reed pipes of the organ, where the air, say they, is put in motion by the trembling reed. But this explanation is wrong in all its parts; even in the reed pipes of an organ, the air is *not* put in motion by the reeds. They are indeed the *occasions* of its musical undulation, but they do not *immediately impel* it into those waves. This method (and indeed all methods but the vibrations of wires, bells, &c.) of producing sound is little understood, though it is highly worthy of notice, being the origin of animal voice, and because a knowledge of it would enable the artists to entertain us with sounds hitherto unknown, and thus add considerably to this gift of our Bountiful Father, who has shewn, in the structure of the larynx of the human species, that he intended that we should enjoy the pleasures

of music as a *laborum dulce lenimen*. He has there placed a micrometer apparatus, by which, *after* the other muscles have done their part in bringing the glottis nearly to the tension which the intended note requires, we can easily, and instantly, adjust it with the utmost nicety.

We trust, therefore, that our readers will indulge us while we give a very cursory view of the manner in which the tremulous motion of the glottis, or of a reed in an organ pipe, produces the sonorous undulations with a constant or uniform frequency, so as to yield a musical note.

If we blow through a small pipe or quill, we produce only a whizzing or hissing noise. If, in blowing, we shut the entry with our tongue, we hear something like a solid blow or tap, and it is accompanied with some faint perception of a musical pitch, just as when we tap with the finger on one of the holes of a flute when all the rest are shut. We are then sensible of a difference of pitch, according to the length of the pipe; a longer pipe or quill giving a graver sound. Here, then, is like the *beginning* of a sonorous undulation. Let us consider the state of the air in the pipe: It was filled by a column of air, which was moving forward, and would have been succeeded by other air in the same state. This air was therefore nearly in its state of natural density. When the entry is suddenly stopped by the tongue, the included air, already in motion, continues its motion. This it cannot do without growing rarer, and then it is no longer a balance for the pressure of the atmosphere. It is therefore retarded in its motion, totally stopped (being in a rarefied state), and is then pressed back again. It comes back with an accelerated motion, and recovers its natural density, while the state of rarefaction goes forward through the open air like any other aërial pulse. Its motions are somewhat, but not altogether, like that of a spiral wire, which has been in like manner moving uniformly along the pipe, and has been stopped by something catching hold of its hindermost extremity. This spring, when thus catched behind, stretches it-

self a little, then contracts *beyond* its natural state, and then expands again, quivering several times. It can be demonstrated that the column of air will make but one quiver. Suppose this accomplished in the hundredth part of a second, and that at that instant the tongue is removed for the hundredth part of a second, and again applied to the entry of the pipe. It is plain that this will produce such another pulse, which will join to the former one, and force it out into the air, and the two pulses together will be like two pulses produced by the vibration of a cord. If, instead of the tongue, we suppose the flat plate of an organ reed to be thus alternately applied to the hole and removed, at the exact moments that the renewals of air are wanted, it is plain that we shall have *sonorous* undulations of *uniform* frequency, and therefore a musical note. This is the way in which reeds produce their effect, not by *impelling* the air into alternate states of motion to and fro, and alternate strata of rarefied and condensed air, but by giving them time to acquire this state by the combination of the air's elasticity with its progressive motion.

The adjustment of the succeeding puff of air to the pulse which precedes it, so that they may make one smooth and regular pulse, is more exact than we have yet remarked; for the stoppage of the hole not only *occasions* a rarefaction *before it*, but by checking the air which was just going to enter, makes a condensation *behind the door* (so to speak); so that, when the passage is again opened, the two parcels of air are fitted for supporting each other, and forming one pulse.

Suppose, in the next place, that the reed, instead of completely shutting the hole each time, only half shuts it. The same thing must still happen, although not in so remarkable a degree. When the passage is contracted, the supply is diminished, and the air now in the pipe must rarefy, by advancing with its former velocity. It must therefore retard; by retarding, regain its former density; and the air, not yet

got into the pipe, must condense, &c. And if the passage be again opened or enlarged in the proper time, we shall have a complete pulse of condensed and rarefied air; and this must be accompanied by the beginning of a musical note, which may be continued like the former.

This will be a softer or more mellow note than the other; for the condensed and the rarefied air will not be so suddenly changed in their densities. The difference will be like the difference of the notes produced by drawing a quill along the teeth of a comb, and that produced by the equally rapid vibrations of a wire. For let it be remarked here, that musical notes are by no means confined, as theorists commonly suppose, to the regular cycloidal agitations of air, such as are produced by the vibrations of an elastic cord; but that any crack, snap, or noise whatever, when repeated with sufficient frequency, becomes *ipso facto* a musical sound, of which we can tell the pitch or note. What can be less musical than the solitary cracks or snaps made by a stiff door when very slowly opened? Do this briskly, and the creak changes to a chirp, of which we can tell the note. The sounds will be harsh or smooth, according as the snaps of which they are composed are abrupt or gradual.

This distinction of sounds is most satisfactorily confirmed by experiment. If the tongue of the organ reed is quite flat, and if, in its vibrations, it apply itself to the whole margin of the hole at once, so as completely to shut it (as is the case in the old-fashioned regal stop of the organ), the note is clear, smart, and harsh or hard: but if the lips of the reed are curved, or the tongue properly bent backward, so that it applies itself to the edges of the hole *gradatim*, and never completely shuts the passage, the note may have any degree of mellow sweetness. This remark is worth the attention of the instrument-makers or organ-builders, and enables them to vary the VOICE of the organ at pleasure. We only mention it here as introductory to the explanation of the sounds of the trumpet.

We trust that the reader now perceives how the air, proceeding along a pipe, may be put in the state of alternate strata of condensed and rarefied air, the particles, in the mean time, proceeding along the pipe with a very moderate velocity; while the *state of undulation* is propagated at the rate of eleven or twelve hundred feet in a second; just as we may sometimes see a stream of water gliding gently down a canal, while a wave runs along its surface with much greater rapidity.

It will greatly assist the imagination, if we compare these aerial undulations with the undulations of water in an open canal. While the water is flowing smoothly along, suppose a sluice to be thrust up from the bottom quite to the surface, or beyond it. This will immediately cause a depression on the lower side of the sluice, by the water's going along the canal, and a heaping up of the water on the other side. By properly timing the motion of this sluice up and down, we can produce a series of connected waves. If the sluice be not pushed up to the surface, but only one-half way, there will be the same succession of waves, but much smoother, &c. &c.

It is in this state, though not by such means, that the air is contained in a sounding trumpet. It is not brought into this state by any tremor of the lips. The trumpeter sometimes feels such a tremor; but whenever he feels it, he can no longer sound his note. His lips are painfully tickled, and he must change his manner of winding.

When blowing with great delicacy and care, the deepest notes of a French horn, or trombone, we sometimes can feel the undulations of the air in the pipe distinctly fluttering and beating against the lips; and it is difficult to hinder the lips from being affected by it: but we feel plainly, that it is not the lips which are fluttering, but the air before them. We feel a curious instance of this when we attempt to whistle in concert. If our accompanier intonates with a certain degree of incorrectness, we feel something at our own lips

which makes it impossible to utter the intended note. This happens very frequently to the person who is whistling the upper note of a greater third. In like manner, the undulations in a pipe re-act on the reed, and check its vibrations. For if the dimensions of a pipe are such that the undulations formed by the reed cannot be kept up in the pipe, or do not suit the length of the pipe, the reed will either not play at all, or will vibrate only in starts. This is finely illustrated by a beautiful and instructive experiment. Take a small reed of the *vox humana* stop of an organ, and set it in a glass foot, adapted to the windbox of the organ. Instead of the common pipe above it, fix on it the sliding tube of a small telescope. When all the joints are thrust down, touch the key, and look attentively to the play of the reed. While it is sounding, draw out the joints, making the pipe continually longer. We shall observe the reed thrown into strange fits of quivering, and sometimes quite motionless, and then thrown into wide sonorous vibrations, according as the *maintainable* pulse is commensurate or not with the vibrations of the reed. This plainly shews that the air is not impelled into its undulations by the reed, but that the reed accommodates itself to the undulations in the pipe.

We acknowledge that we cannot explain with distinctness in what manner the air in a trumpet is first put into musical undulations. We see that it is only in very long and slender tubes that this can be done. In short tubes, of considerable diameter, like the cow-herd's horn, we obtain only one or two very indistinct notes, of which it is difficult to name the pitch; and this requires great force of blast; whereas, to bring out the deep notes of the French horn, a very gentle and well-regulated blast is necessary. The form of the lips, combined with the force of the blast, form all the notes. But this is in a way that cannot be taught by any description. The performer learns it by habit, and *feels* that the instrument leaps into its note without him, when he gradually varies his blast, and continues sounding the

same note; although he, in the mean time, makes some small change in his manner of blowing. This is owing to what Mr. Bernoulli observed. The tube is suited only to such pulses, and can only maintain such pulses as correspond to aliquot parts of its length: and when the embouchure is very nearly, but not accurately, suited to a particular note, that note *forms itself* in the tube, and, resting on the lips, brings them into the form which can maintain it with ease. We have a proof of this when we attempt to sound the note corresponding to one-seventh of the length. Not having a distinct notion of this note, which makes no part of our scale of melody, we cannot easily prepare for it in the way that habit teaches us to prepare for the others: whereas, from what we shall see presently, the notes *one-sixth* and *one-eighth* are both familiar to the mind, and easily produced. When, therefore, we attempt to produce the note *one-seventh*, we slide, against our will, into the *one-sixth* or *one-eighth*.

Nor can we completely illustrate the formation of musical pulses by waves in water. A canal is equally susceptible of every height and length of progressive waves; whereas we see that a certain length of tube will maintain only certain determined pulses of air.

We must therefore content ourselves for the present with having learned, by means of the reed pipes, how the air may exist progressively in a tube, in an alternate state of condensation and rarefaction; and we shall now proceed to consider how this state of the air is related to the length of the tube. And here we can do no more than give an outline of Mr. Bernoulli's beautiful theory of flutes and trumpets, but without a mathematical examination of the particular motions. We can, however, shew, with sufficient evidence, how the different notes are produced from the same tube. It requires, however, a very steady attention from the reader to enable him to perceive how the different por-

tions of this air act on each other. We trust that this will now be given.

The conditions which must be implemented, in order to maintain a musical pulse, are two: 1. That the vibrations of the different plates of air be performed in equal times, otherwise they would all mix and confound each other. 2. That they move altogether, all beginning and all ending at the same instant. It does not appear that any other state of vibration can exist and be maintained.

The column of air in a tube may be considered as a material spring (having weight and inertia). This spring is compressed and coiled up by the pressure of the atmosphere. But in this coiled state it can vibrate in its different parts, as a long spiral wire may do, though pressed a little together at the ends. It is evident that the air within a pipe, shut at both ends, may be placed in such a situation, *in a variety of ways*, that it will vibrate in every part, in the same manner as a chord of the same length and weight, strained by a force equal to the pressure of the atmosphere. Thus, in a shut pipe AB (Plate VII. fig. 1.), suppose a harmonic curve ACB, or a wire of the same weight with the air, throwing itself into the form of this curve. The force which impels the point C to the axis is to that which impels the point *c* as CE to *c e*. Now, suppose the air in this pipe divided into parallel strata or plates, crossing the tube like diaphragms. In order that these may vibrate in the same manner (not across the tube, but in the direction of its axis), all that is necessary for the moment is, that the excess of the pressure of the stratum *d d* above that of the stratum *f f* may be to the excess of the pressure of DD above that of FF as *c e* to CE. In this case, the stratum *e e* will be accelerated in the direction *e f*, and the stratum EE is accelerated in the same direction, and in the due proportion. Now this may be done in an infinite variety of ways for a single moment. It depends, not on the absolute density, but on the *variation* of density; because the pressure

by which a particle of air is urged in any direction arises from the *difference* of the distances of the adjoining particles on each side of it. But in order to *continue* this vibration, or in order that it may obtain at once in the whole pipe, this variation of density must continue, and be according to some connected law. This circumstance greatly limits the ways in which the vibration may be kept up. Mr. Bernoulli finds that the isochronism and synchronism can be maintained in the following manner, and in no other that he could think of:

Let AB (Plate VII. fig. 2.) be a cylindrical pipe, shut at A, and open at B. Then, in whatever manner the sound is produced in the pipe, the undulations of the contained air must be performed as follows: Let *a a* be a plate of air. This plate will approach to, and recede from, the shut end A, vibrating between the situations *b b* and *c c*, the whole vibration being *b c*, and the plate will vibrate like a pendulum in a cycloid. The greater we suppose the excursions *a b*, *a c*, the louder will the sound be; but the duration of them all must be the same, to agree with the fact that the tone remains the same. The motion will be accelerated in approaching to *a a* from either side, and retarded in the recess from it. Let us next consider a plate $\alpha\ \alpha$, more remote from A. It must make similar vibrations from the situation $\beta\ \beta$ to the situation $\gamma\ \gamma$. But these vibrations must be greater in proportion as the plate is farther from A. It cannot be conceived otherwise: For suppose the plate $\alpha\ \alpha$ to make the *same* excursions with *a a*, and that the rest do the same. Then they will all retain the same distances from each other; and thus there will be no force whatever acting on any particles to make them vibrate. But if every particle make excursions proportional to its distance from A, the *variation* of density will, in any instant, be the same through the whole pipe, and each particle in the vibrating plate $\beta\ \beta$ will be accelerated or retarded in proportion to its distance from A; while the accelerations and retardations over all

will, in any instant, be proportional to the distance of each particle from its place of rest. All this will appear to the mathematician, who attentively considers any momentary situation of the particles. In this manner all the particles will support each other in their vibrations.

It follows from this description that the air in the tube is alternately rarefied and condensed. But these changes are very different in different parts of the tube. They must be greatest of all at A; because, while all the plates approach to A, they concur in condensing the air immediately adjoining to A; while the air in *a a* and *a a* is less condensed by the action of the plates beyond it. The air at B is always of its natural density, being in equilibrio with the surrounding air. At B, therefore, there is a small parcel of air, of its natural density, which is alternately going in and out.

This account is confirmed by many facts. If the bottom of the pipe be shut by a fine membrane, stretched across it like a drumhead, with a wire stretched over it, either externally or internally, in the same manner as the catgut is stretched across the bottom of a drum, it will be thrown into strong vibrations, making a very loud noise, by rattling against the cross wire. The same thing happens if the membrane be pasted over a hole close to the bottom, leaving a small space round the edge of the hole without paste, so that the membrane may play out and in, and rattle on the margin of the hole. This also makes a prodigious noise. Now, if the membrane be pasted on a hole far from the bottom, the agitations will be much fainter; and when the hole is near the mouth of the pipe, there will be none.—When a pipe has its air agitated in this manner, it is giving the lowest note of which it is susceptible.

Let us next consider a pipe open at both ends. Let CB (Plate VII. fig. 3.) be this pipe. It is plain that, if there be a partition A in the middle, we shall have two pipes AB, AC, each of which may undulate in the manner now described, if the undulations in each be in opposite directions.

It is evidently possible, also, that these undulations may be the same in point of strength in both, and that they may begin in the same instant. In this case, the air on each side of the partition will be in the same state, whether of condensation or rarefaction, and the partition A itself will always be in equilibrio. It will perfectly resemble the point C of the musical cord BFCGH (Plate VII. fig. 6.), which is in equilibrio between the vibrating forces of its two parts. In the pipe, the plates of air on each side are either both approaching it, or both receding from it, and the partition is either equally squeezed from both sides, or equally drawn outwards. Consequently this partition may be removed, and the parcels of air on each side will, in any instant, support each other. There seems no other way of conceiving these vibrations in open pipes which will admit of an explanation by mechanical laws. The vibrations of all the plates must be obtained without any mutual hindrance, in order to produce the tone which we really hear; and therefore such vibrations are impressed by Nature on each plate of air.

But if this explanation be just, it is plain that this pipe CB must give the same note with the pipe AB (Plate VII. fig. 2.) of half the length, shut at one end. But the sound, being doubled, with perfect consonance, must be clear, strong, and mellow. Now this is perfectly agreeable to observation; and this fact is an unequivocal confirmation of the justness of the theory. If we take a slender pipe, about six inches long and one half of an inch wide, shut at one end, and sound it by blowing across its mouth, as we whistle on the pipe of a key, or across a hole that is close to the mouth, and formed with an edge like the sound-hole of a German flute, we shall get a very distinct and clear tone from it. If we now take a pipe of double the length, open at both ends, and blow across its mouth, we obtain the same note, but more clear and strong. And the note produced by blowing across the mouth is not changed by a hole made

exactly in the middle, in respect of its musical pitch, although it is greatly hurt in point of clearness and strength. Also a membrane at this hole is strongly agitated. All this is in perfect conformity to this mechanism.

Thus we have, in a great measure, explained the effect of an open and a shut pipe. The shut pipe is always an octave, graver than an open pipe of the same length; because the open pipe is in unison with a shut pipe of half the length.

Let AC (Plate VII. fig. 4.) be a pipe shut at both ends. We may consider it as composed of two pipes AB, BC, stopped at A and C, and open at B. Undulations may be performed in each half, precisely as in the pipe AB of fig. 2.; and they will not, in the smallest degree, obstruct each other, if we only suppose that the plates in each half are vibrating at once in the *same* direction. The condensation in AB will correspond with the rarefaction in BC, and the middle parcel B will maintain its natural density, vibrating to and again, across the middle; and two plates $a\ a$, $\alpha\ \alpha$, which are equally distant from B, will make equal excursions in the same direction.

We may produce sound in this pipe by making an opening at B. Its note will be found to be the same with that of BC of fig. 3. or of AB of fig. 2.

In the next place, let a pipe, shut at one end, be considered as divided into any odd number of equal parts, and let them be taken in pairs, beginning at the stopped end, so that there may be an odd one left at the open end. It is plain that each of these pairs may be considered as a pipe stopped at both ends, as in fig. 4.

For the partitions will, of themselves, be in equilibrio, and may be removed, and vibrations may be maintained in the whole, consistent with the vibration of the odd part at the open end; and these vibrations will all support each other, and the plates of air which are at the points of division will

remain at rest. Conceive the pipe AB of fig. 2. to be added to the pipe AC of fig. 4. the part A of the first being joined to A of the other. Now, suppose the vibrations to be performed in both, in such a manner that the simultaneous undulations on each side of the junction may be in opposite directions. It is plain that the partition will be in equilibrio, and may be removed; and the plate of air will perform the same office, being alternately the middle plate of a condensed and of a rarefied parcel of air. The two pipes CA, AB will together give the same note that AB would have given alone, but louder.

In like manner may another pipe, equal to AC, be joined to the shut end of this compound pipe, as in fig. 5. and the three will still give the same note that AB would have done alone.

And in the same manner may any number of pipes, each equal to AC, be added, and the whole will give still the same note that AB would have given alone.

Hence it legitimately follows, that if the undulations can be once begun in this manner in a pipe, it may give either the sound competent to it, as a single pipe AB (Plate VII. fig. 2.); or it may give the sound competent to a pipe of $\frac{1}{3}$d, $\frac{1}{5}$th, $\frac{1}{7}$th, &c. of its length; the undulations in each part AB, BC, CD, maintaining themselves in the manner already described. This seems the only way in which they can be preserved, both isochronous and synchronous.

It is known that the gravest tones of pipes are as the lengths of the pipes, or the frequency of the undulations are inversely as their lengths. (This will be *demonstrated* presently). Therefore these accessory tones should be as the odd numbers 3, 5, 7, &c. and the whole tones, including the fundamental, should form the progression of the odd numbers 1, 3, 5, 7, &c.

This is abundantly confirmed by experiment. Take a German flute, and stop all the finger-holes. The flute, by

gradually forcing the blast, will give the fundamental, the 12th, the 17th, the 21st, &c. *

Again, let AD (Plate VII. fig. 6.) represent the length of a pipe. Construct on AD an harmonic curve AEBFC GHD, in such a manner that HD may be $\frac{1}{2}$ AB, $= \frac{1}{2}$ BC, $= \frac{1}{2}$ CH. The small ordinates $m\ n$ will express the total excursion of the plates of air at the points $m\ m$, &c. and those ordinates which are above the axis will express excursions on one side of the place of rest, and the ordinates below will mark the excursions in the opposite directions, in the same manner as if this harmonic curve were really a vibrating cord. These excursions are nothing in the points A, B, C, H, and are greatest at the points E, F, G, D, where the little mass of air retains its natural density, and travels to and again, condensing the air at B, or rarefying it, according as the parcels E and F are approaching to or receding from each other. The points A, B, C, H, may be called Nodes, and the parts E, F, G, D, may be called Bights or Loops. This represents very well to the eye the motion of the plates of air. The density and velocity need not be minutely considered at present. It is enough that we see that when the density is increasing at A, by the ap-

* A little reflection will teach us that these tones will not be perfectly in the scale. A certain proportion between the diameter and length of the pipe produces a certain tone. Making the pipe wider or smaller flattens or sharpens this tone a little, and also greatly changes its clearness. Organ-builders, who have tried every proportion, have adopted what they found best. This requires the diameter to be about $\frac{1}{11}$th or $\frac{1}{12}$th of the length. Therefore, when we cause the same pipe to sound different notes, we neglect this proportion; and the notes are false, and even very coarse, when we produce one corresponding to a very small portion of the pipe. For a similar reason, Mr. Lambert found that, in order to make his pitch-pipe sound the octave to any of its notes, it was not sufficient to shorten its capacity one-half by pushing down the piston; he found that the part remaining must be less than the part taken off by a fixed quantity $1\frac{5}{12}$ inches. Or, the length which gave any note being x, the length for its octave must be $\frac{x - 1\frac{5}{12}}{2}$.

proach of the parcel E, it is diminishing at B by the recess of E and F; and increasing at C, by the approach of F and G, and diminishing at H by the recess of G. In the next vibration it will be diminishing at A and C, and increasing at B and H. And thus the alternate nodes will be in the same state, and the adjoining nodes in opposite states.

The reader must carefully distinguish this motion from the undulatory motion of a pulse, investigated by Newton. That undulation is going on at the same time, and is *a result* of what we are now considering, and the cause of our hearing this undulation. The undulation we are now considering is the original agitation, or rather it is the SOUNDING BODY, as much as a vibrating string or bell is; for it is not the trumpet that we hear, but the air trembling in the trumpet. The trumpet is performing the office, not of the string, but of the pin and bridge on which the string is strained. This is an important remark in the philosophy of musical sounds.

There is yet another set of notes producible from a pipe besides those which follow in the order of frequency 1, 3, 5, 7, &c.

Suppose a pipe open at both ends, sounding by blowing across the end, and undulating, as already described, with a node in the middle A (Plate VII. fig. 3.) If we still express the fundamental note of the pipe AB of Plate VII. fig. 2. by 1, it is plain that the fundamental of an open pipe of the same length will have the frequency of its undulations expressed by 2; because an open pipe of twice the length of AB (Plate VII. fig. 2.) will be 1, the two pipes AB (fig. 2.), and CB (fig. 3.), being in unison.

But this open pipe may be made to undulate in another manner; for we have seen that AB of Plate VII. fig. 2, joined to CA of fig. 4. may sound altogether when the partition A is removed, still giving the note of AB (fig. 2.) Let such another as AB (fig. 2.) be added to the end C, and let the partition be removed. The whole may still undu-

late, and still produce the same note; that is, a pipe open at both ends may sound a note which is the fundamental of a pipe like AB (fig. 2), but only one-fourth of its length. The pipe CB of fig. 3. may thus be supposed to be divided into four equal parts, CE, EA, AF, FB, of which the extreme parts EC and FB contain undulations similar to those in AB (fig. 2.); and the two middle parts contain undulations like those in CA (fig. 4.). The partitions at E and F may be removed, because the undulations in EC and EA will support each other, if they are in opposite directions; and those in FB and FA may support each other in the same manner.

It must here be remarked, that in this state of undulation the direction of the agitations at the two extremities is the same; for in the middle piece EF the particles are moving one way, condensing the air at E, while they rarify it at F. Therefore, while the middle parcel is moving from E towards F, the air at B must be moving towards F, and the air at C must be moving from E. In short, the air at the two extremities must, in every instant, be moving in the opposite direction to that of the air in the middle.

In like manner, if the pipe CB of Plate VII. fig. 3. be divided into six parts, the two extreme parts may undulate like AB of fig. 2. and the four inner parts may undulate like two pipes, such as CA of fig. 4. and the whole will give the sound which makes the fundamental of a pipe of one-sixth of the length, or having the frequency 6.

We may remark here, that the simultaneous motion of the air at the extremities is in opposite directions, whereas in the last case it was in the same direction. This is easily seen; for as the partition which is between the two middle pieces must always be in equilibrio, the air must be coming in or going out at the extremities together. This circumstance must give some sensible difference of character to the sounds 4 and 6. In the one, the agitations at each end of the tube are in the same direction, and in the other they

are in the opposite. Both produce pulses of sound which are conveyed to the ear. Thus we see that the air in a pipe open at both ends may undulate in two ways. It may undulate with a node in the middle, giving the note of AB (Plate VII. fig. 2.), or of its 3d, 5th, 7th, &c. past; and it may undulate with a loop or bight in the middle, sounding like $\frac{1}{2}$, $\frac{1}{4}$, $\frac{1}{6}$, &c. of AB, fig. 2.

In like manner may this pipe produce sounds whose frequency are expressed by 8, 10, &c. and proceed as the even numbers.

This state of agitation may be represented in the same way that we represented the sounds 1, 3, 5, &c. by constructing on AM (Plate VII. fig. 7.) an harmonic curve, with any number of nodes and loops. Divide the parts AF, FD, DE, EM, equally in C, O, P, B. CB will correspond to the pipe, and the ordinates to the curve GFHDLEN will express the excursions of the plates of air.

If the pipe gives its fundamental note, its length must be represented by CO, and the undulations in it will resemble the vibrations of part CO of a cord, whose length AD is equal to 2CO, and which has a node in F.

If the pipe is sounding its octave, it will be represented by CP, and its undulations will resemble the vibrations of a cord CP, whose length AE is $\frac{3}{2}$ of CP, having nodes at F and D, &c. &c.

We can now see the possibility of such undulations existing in a pipe as will be permanent, and produce all the variety of notes by a mere change in the manner of blowing, and why these notes are in the order of the natural numbers, precisely as we observe to happen in winding the trumpet or French horn. We have, 1*st*, the fundamental expressed by 1; then the octave 2; then the 12th 3; the double octave 4; then the third major of that octave 5, or 17th of the fundamental; then the octave of the 12th, or the 5th of this double octave, = 6. We then jump to the

triple octave 8, without producing the intermediate sound corresponding to $\frac{1}{7}$th of the pipe. With much attention we can hit it; and it is a fact that a person void of musical ear stumbles on it as easily as on any other. But the musician, finding this sound begin with hum, and his ear being grated with it, perhaps thinks that he is mistaking his embouchure, and he slides into the octave. After the triple octave, we easily hit the sounds corresponding to $\frac{1}{9}$ and $\frac{1}{10}$, which are the 2d and 3d of this octave. The next note $\frac{1}{11}$ is sharper than a just 4th. We easily produce the note 12, which is a just 5th; 13 is a false 6th; 14 is a sound of no use in our music, but easily hit; 15 and 16 give the exact 7th and 8th of this octave.

Thus, as we ascend, we introduce more notes into every octave, till at last we can nearly complete a very high octave; but in order to do this with success, and tolerable readiness, we must take an instrument of a very low pitch, that we may be able nearly to fill up the steps of the octave in which our melody lies. Few players can make the French horn or trombone sound its real fundamental, and the octave is generally mistaken for it. The proof of this is, that most players can give the 5th of the lowest note that they are able to produce; whereas the 5th of the real fundamental cannot be uttered. Therefore that lowest note is not the fundamental, but the octave to the fundamental.

Few performers can sound even this second octave on a short instrument, such as the ordinary military trumpet; and what they imagine to be the fundamental sound of this instrument is the double octave above it. This appears very strange; and it may be asked, how we know what is really the fundamental note of a trumpet? The answer to this is to be obtained only by demonstrating, on mechanical principles, what is the frequency of undulation corresponding to a given length of pipe. This is a proposition equally fundamental with its corresponding one in the theory of musical cords; but we have reserved it till now, because

many readers would stop short at such an investigation, who are able to understand completely what we have now delivered concerning the music of the trumpet.

Suppose therefore a pipe shut at both ends, and that the whole weight of the contained air is concentrated in its middle point, the rest retaining its elasticity without inertia; or (which is a more accurate conception), let the middle point be conceived as extending its elasticity to the two extremities of the pipe, being repelled from each by a force inversely as the distance. Let the length of this pipe be L. This may also express the weight of the middle plate of air, which will always be proportional to the length of the pipe, because all is supposed to be concentrated there. Let E be the elasticity of the air. This must be measured by the pressure of the atmosphere, or by the weight of the column of mercury in the barometer. Perhaps the rationale of this will be better conceived by some readers by considering E as the height of a homogeneous atmosphere. Then it is plain that E is to L as the weight of this atmospheric column to the weight of the column of the same air which fills the pipe whose length is L. Then it is also plain that E is to L as the external pressure, and consequently, as the elasticity which supports that pressure is to the weight or inertia of the matter to be moved. Let this middle plate or diaphragm be withdrawn from its place of rest to the very small distance a. The elasticity or repulsion will be augmented on one side and diminished on the other; and the difference between them is the only force which impels the diaphragm toward the middle point, and causes it to vibrate, or produces the undulation. It is plain that the repulsion on one side is $\frac{\frac{1}{2}L}{\frac{1}{2}L-a} \times E$, or $\frac{L}{L-2a} E$ (for $\frac{1}{2}L - a : \frac{1}{2}L = E : \frac{\frac{1}{2}LE}{\frac{1}{2}L-a}$), and the repulsion on the other side is $\frac{\frac{1}{2}L}{\frac{1}{2}L+a} \times E$, or $\frac{L}{L+2a} E$. The difference of these repulsions is

$E \times L \times \frac{4a}{L^2 - 4a^2}$. But as we suppose a exceedingly small in comparison with L, this difference, or the accelerating force, may safely be expressed by $E \frac{4a}{L}$, or $4a \frac{E}{L}$.

Hence we deduce, in the first place, that the undulations will be isochronous, whether wide or narrow; because the accelerating force is always proportional to the distance a from the middle point.

Now, let a pendulum, whose quantity of matter is L, and length a, be supposed to vibrate in a cycloid by the force $\frac{4a}{L}E$ or $\frac{4E}{L}a$. It must perform its vibrations in the same time with the plate of air; because the moving force, the matter to be moved, and the space along which they are to be similarly impelled, are the same in both cases. Let another pendulum, having the same quantity of matter L, vibrate by its weight L alone. In order that these two pendulums may vibrate in equal times, their lengths must be as the accelerating forces. Therefore we must have $\frac{4E}{L}$ $a : L = a : \frac{aL^2}{4Ea}, = \frac{L^2}{4E}$, which is therefore the length of the synchronous pendulum.

Now, a cord without weight and inertia, but loaded with the weight L at its middle point, and strained by a weight E, and drawn from the axis to the distance a, is precisely similar in its motion to the diaphragm we are now considering, and must make its oscillations in the same time.

This is applicable to any number of plates of air, by substituting in the cord a loaded point for each of the plates for when the case is thus changed, both in the pipe and the cord, the space to be passed over by the plate of air bear the same proportion to a, which is passed over by the whol air concentrated in the middle point, which the space to b passed over by the corresponding loaded point of the cor

bears to that passed over by the whole matter of the cord concentrated in the middle point; and the same equality of ratios obtains in the accelerating forces of the plate of air and the corresponding loaded point of the cord. Suppose, then, a pipe divided into 2, 3, 4, &c. equal parts, by 1, 2, 3, diaphragms, each of which contains the air of the intervening portion of the pipe, the whole weight L being equally divided among them. If there be but one diaphragm, its weight must be L; if two, the weight of each must be $\frac{1}{2}$ L; if three, the weight of each must be $\frac{1}{3}$ L; and so on for any number.

By considering this attentively, we may infer, without farther investigation, what will be the undulations of all the different plates of air in a pipe stopped at both ends. We have only to compare it with a cord similarly divided and loaded. Increase the number of loaded points, and diminish the load on each, continually—it is evident that this terminates in the case of a simple cord, with its matter uniformly diffused; and a simple pipe, with its air also uniformly diffused over its whole length.

Therefore, if we take an elastic cord, and stretch it by such a weight that the extending weight may bear the same proportion to the accelerating force acting on the whole matter concentrated in its middle point, which the elasticity of the air bears to its accelerating force acting on the whole matter concentrated at the mouth of an open pipe, sounding its fundamental note, the cord and the air will vibrate in the same time. Moreover, since the proportion between the vibrations of a cord so constituted, and those of a cord having its matter uniformly diffused, is the same with the proportion between the undulations in a pipe so constituted, and those of a pipe in which the air is uniformly diffused—it is plain that the vibrations of the cord and of the pipe in their natural state will also be performed in equal times.

We look on this as the easiest way of obtaining a distinct perception of the authority on which we rest our knowledge

of the absolute number of undulations of the air in a pipe o given length. It may be obtained directly; and Danie Bernoulli, Euler, and others, have given very elegant solu tions of this problem, without having recourse to the analo gy of the vibrations of cords and undulations of a column o air. But it requires more mathematical knowledge tha many readers are possessed of who are fully able to follo out this analogical investigation.

Let us therefore compare this theory with experiment What we call an open pipe of an organ is the same whic we, in this theory, have considered as a pipe open at bot ends; for the opening at the foot, which the organ-builder call the VOICE of the pipe, is equivalent to a complete open ing. The aperture, and the sharp edge which divides th wind, may be continued all round, and the wind admitte by a circular slit, as is represented in Plate VII. fig. 10. W have tried this, and it gives the most brilliant and clear tone we ever heard, far exceeding the tones of the organ. A open organ pipe, therefore, when sounding its fundamenta note, undulates with one node in its middle, and its undula tions are analogous, in respect of their mechanism, to th vibrations of a wire of the same length, and the same weight with the column of air in the pipe, and stretched by a weigh equal to that of a column of the same air, reaching to th top of a homogeneous atmosphere, or equal to the weigh of a column of mercury as high as that in the barometer.

Dr. Smith (see *Harmonics*, 2d edit. p. 193.) found that a brass wire, whose length was 35,55 inches, and weight 3 troy grains, and stretched by 7 pounds avoirdupois, or 4900 grains, was in perfect unison with an open organ pipe whos length was 86,4 inches.

Now 86,4 inches of this wire weighs 75,34 grains. Whe the barometer stands at 30 inches, and the thermometer a 55° (the temperature at the time of the experiment), th height of a homogeneous atmosphere is 332640 inches. Thi has the same proportion to the length of the pipe which th

pressure of the atmosphere has to the weight of the column of air contained in the pipe.

Now 86,4 : 332640 = 75,34 : 290060. This wire, therefore, should be stretched (if the theory be just) by 290060 grains, in order to be unison with the other wire, and we should have . . . $35{,}55^2 : 86{,}4^2 = 49000 : 290060$
But, in truth, . . . $35{,}55^2 : 86{,}4^2 = 49000 : 289430$
The difference is 630
The error scarcely exceeds $\frac{1}{300}$, and does not amount to an error of one vibration in a second.

We must therefore account this theory as accurate, seeing that it agrees with experiment with all desirable exactness.

We may also deduce from it a very compendious rule for determining the absolute number of aerial pulses made by an open pipe of any given length. When considering the vibrations of cords, we found that the number of vibrations made in a second is $\sqrt{\frac{386\,E}{LW}}$, where E is the extending weight, W the weight of the cord, and L its length. Let H be the height of a homogeneous atmosphere. We have its weight $= \frac{HW}{L}$, $= E$. Therefore substituting $\frac{HW}{L}$ for E in the above formula, we have the number of aerial pulses made per second $= \sqrt{\frac{386\,H}{L^2}}$, or $= \frac{\sqrt{386\,H}}{L}$. Now $\sqrt{386\,H}$, computed in inches, is 11331. Therefore, if we also measure the length of the pipe L in inches, the pulses in a second are $= \frac{11331}{L}$. Thus, in the case before us, $\frac{11331}{86{,}4} = 131{,}12$, or this pipe produces 131 pulses in a second. Dr. Smith found by experiment that it produced 130,0, differing only about $\frac{1}{6}$th of a pulse*.

* "In the preceding rule, Professor Robison considers the number of pulses as equal to the number of vibrations performed by the air in the pipe, and determines the number of vibrations by extending to an open pipe the

We see that the pitch of a pipe depends on the height of the homogeneous atmosphere. This may vary by a change of temperature. When the air is warmer it expands, and the weight of the induced column is lessened, while it still carries the same pressure. Therefore the pitch must rise. Dr. Smith found his organ a full quarter tone higher in summer than in winter. The effect of this is often felt in concerts of wind instruments with stringed instruments. The heat which sharpens the tone of the first flattens the last. The harpsichord soon gets out of tune with the horns and flutes.

Sir Isaac Newton, comparing the velocity of sound with the number of pulses made by a pipe of given length, observed that the length of a pulse was twice the length of the open pipe which produced it. Divide the space passed over in a second by the number of pulses, and we obtain the length of each pulse. Now it was found that a pipe of 21.9 inches produced 262 pulses. The velocity of sound (as computed by the theory on which our investigation of the undulations in pipes procceds) is 960 feet. Now $\frac{960 \times 12}{262} = 44$ inches very nearly, the half of which is 22, which hardly differs from 21,9. The difference of this theoretical velocity of sound, and its real velocity 1142 feet per second, remains still to be accounted for. We may just observe here, that when a pipe is measured, and its length called 21,9, we do really allow it too little. The voice-hole is equivalent to a portion, not inconsiderable of its length, as appears very clearly from the experiments of Mr. Lambert on a variabl

formula which he had previously found for the vibrations of a string. No as that formula expresses the single vibrations of a string, the extension of must express the number of single vibrations of the air in a pipe, and consequently give double the number of pulses. If this rule be applied to the e ample given in the second next paragraph, it will be found to give double t number of pulses mentioned there by that philosopher."—EDINBURGH ENCYCLOPÆDIA, Art. ACOUSTICS, Chap. III.

pitch-pipe, and on the German flute, recorded in the Berlin Memoirs for 1775. He found it equivalent to $\frac{1}{6}$th; and this is sufficient for reconciling these measures of a pulse with the real velocity of sound.

The determination which we have given of the undulations of air in an organ pipe is indirect, and is but a sketch of the beautiful theory of Daniel Bernoulli, in which he states with accuracy the precise undulation of each plate of air, both in respect of position, density, velocity, and direction of its motion. It is a pleasure to observe how the different equations coincide with those which express the vibrations of an elastic cord. But this would have taken up much room, and would not have been suited to the information of many curious readers, who can easily follow the train of reasoning which we have employed.

Mr. Bernoulli applies the same theory to the explanation of the undulations in flutes, or instruments whose sounds are modified by holes in the sides of the pipe. But this is foreign to our purpose of explaining the music of the trumpet. We shall only observe, that a hole made in that part of a pipe where a node should form itself, in order to render practicable the undulations, competent to a particular note, prevents its formation, and in its place we only get such undulations (and their corresponding sounds) as have a loop in that place. The intelligent reader will perceive that this single circumstance will explain almost every phenomenon of flutes with holes; and also the effects of holes in instruments with a reed voice, such as the hautboy or clarionette.

We now see that the sound or musical pitch of a pipe is inversely as its length, in the same manner as in strings. And we learn, by comparing them, that the sound of a trumpet has the same pitch with an open organ pipe of the same length. A French horn, 16 feet long, has the sound C *fa ut*, which is also the sound of an open flute-pipe of that length.

The Trombone, great trumpet, or Sackbut, is an old instrument described by Mersennus and other authors of

the last century. It has a part which slides (air-tight) within the other. By this contrivance the pitch can be altered by the performer as he plays. This is a great improvement when in good hands; because we can thus correct all the false notes of the trumpet, which are very offensive, when they occur in an emphatical or holding note of a piece of music. We can even employ this contrivance for filling up the blanks in the lower octaves.

We must not take leave of this subject without taking notice of another discovery of Mr. Bernoulli's, which is exceedingly curious, and of the greatest importance in the philosophy of music.

Artists had long ago observed that the deep notes of musical instruments are sometimes accompanied by their harmonic sounds. This is most clearly perceived in bells, some of which give these harmonics, particularly the 12th, almost as strong as the fundamental. Musicians, by attending more carefully to the thing, seem now to think that this accompaniment is universal. If one of the finest sounding strings of the bases of a harpsichord be struck, we can hear the 12th very plainly as the sound is dying away, and the 17th major is the last sound that dies away on the ear. This will be rendered much more sensible, if we divide the wire into five parts, and at the points of division tie round it a thread with a fast knot, and cut the ends off very short. This makes the string false indeed by the unequal loading; but, by rendering those parts somewhat less moveable by this additional matter, the portions of the wire between these points are thus jogged, as it were, into secondary vibrations, which have a more sensible proportion to the fundamental vibration. This is still more sensible in the sound of the strings of a violincello when so loaded; but we must be careful not to load them too much, because this would so much retard the fundamental vibration, without retarding the secondary vibrations, that both cannot be maintained together. (*N. B.* This experiment always produces a beat

in the sound).—Listening to a fine sounding flute-pipe of the organ, we can also very often perceive the same thing. Mr. Rameau, and most other theorists in music, now assert that this is the essence of a musical sound, and *necessarily* exists in all of them, distinguishing them from harsh noises. Rameau has made this the foundation of his system of music, asserting that the pleasure of harmony results from the successful imitation of this harmony of Nature. But a little logic should convince these theorists that they must be mistaken. If a note is musical because it has these accompaniments, and by this composition alone is a musical note, what are these harmonics? Are they musical notes? This is granted. Therefore they have the same composition; and a musical note must consist at once of every possible sound; yet we know that this would be a jarring noise. A little mathematics, too, or mechanics would have convinced them. A simple vibration is surely a most possible thing, and therefore a simple sound. No, say the theorists; for though the vibration of the cord may be simple, it produces such undulations in the air as excite in us the perception of the harmonics. But this is a mere assertion, and leaves the question undecided. Is not a simple undulation of the air as possible as the simple vibration of a cord?

It is, however, a very curious thing, that almost all musical sounds really have this accompaniment of the octave, 12th, double octave, and 17th major; for these are the harmonics that we hear.

The jealousy of Leibnitz and of John Bernoulli, and their unfriendly thoughts respecting all the British mathematicians, made John Bernoulli do every thing in his power to lessen the value of Dr. Taylor's investigation of the vibration of a musical cord. Taylor gave him a good opportunity. Perhaps a little vain of his investigation of this abstruse matter, he thought too much of it. He affirmed that the harmonic curve was the essential form of a string

giving a musical note. This was denied, without knowing at first whether it was true or false. But as the analytic mathematics improved, it was at length found that there are an infinity of forms into which an elastic cord can be thrown, which are consistent both with isochronous vibrations, whether wide or narrow, and also with the condition of the whole cord becoming a straight line at once. Euler, D'Alembert, and De la Grange, have prosecuted this matter with great ingenuity, and it is one of the finest problems of the present day.

Daniel Bernoulli, of a very different cast of mind from his illustrious friends, admired both Newton and Taylor; and so far from wishing to eclipse Dr. Taylor by the additions he had made to his theory, tried whether he could not extend Taylor's doctrine as far as the author had said. When he took a review of what he had done while explaining the partial vibrations of musical cords, he thought it very possible that while a cord is vibrating in three portions, with two nodes or points of rest, and sounding the 12th to its fundamental, it might at the same time be also vibrating as a simple cord, and sounding its fundamental note. It was possible, he thought, that the three portions might be vibrating between the four points with a triple frequency, while the two middle nodes were vibrating across the straight line between the two pins; and thus the vibrating cord might be a moveable axis, to which the rapid vibrations of the three parts might always be referred. This was very specious; and when a little more attentively considered, became more probable: for if the cord A p B q C r D (Plate VII. fig. 8.) be vibrating as a 12th to its fundamental AD, the points B and C are in equilibrio. If therefore these two points be laid hold of by hooks, and be drawn aside to β and γ, while the string is yet vibrating, this should not hinder the vibrations. If the hooks be annihilated in an instant, the whole should vibrate between A and D; and this should be in a way very different from the simple vibration.

The question now is, will the cord *continue* to vibrate with the loops $\beta\ s\ \gamma$, $\beta\ q\ \gamma$, &c. in the 900th part of a second (for instance,) while the whole string vibrates from A $\beta\ \gamma$ D to A $\beta'\ \gamma'$ D in the 300th part of a second? or will it at once acquire the form of the simple harmonic curve? The case in which it is most likely to take the latter mode of vibration is when the points β and γ are let go at the instant that each portion of the string is in the middle of its vibration, and therefore forms the line A $\beta\ \gamma$ D. But a moment's consideration will shew us that it cannot do this; for at that instant the point *v*, for instance, which had come from *q*, is moving outwards with a most rapid motion, and therefore will continue to go outward, while β and γ are approaching the axis. The point *w*, on the contrary, is at this moment approaching the axis with a motion equally rapid. They cannot therefore all come to the axis at once, and the vibration must differ greatly from a simple one. On the other hand, let it be supposed that both species of vibrations can be preserved, and that, at the moment of letting go the points β and γ, the cord has the form A $m\ \beta\ q\ \gamma\ n$ D. Then, when β and γ have come to B and C, having made $\frac{1}{2}$ a vibration, the point *m* will be in the axis, having made a vibration downward, and a half vibration upwards, *q*, in like manner, is in the axis, having made a whole vibration upwards, and half a vibration downwards. *n* is like *m*. Thus the whole comes to the axis at once; and in such a manner, that if the points B and C were instantly stopped, the three portions would continue their partial vibrations without any new effort. The result of this compound vibration must be a compound pulse of air, which will excite in us the perception of the fundamental sound and of its 12th. The consequence will be the same if the points β and γ are stopped any where short of the axis; and therefore (said Bernoulli) the string will really vibrate so if not stopped at all.

But this was refused by Euler, who observed that in the points β and γ of contrary flexure, having no curvature,

there can be no accelerating force. This caused Bernoulli to attempt a direct investigation, examining minutely the curvatures and accelerating forces in the different points.

He had the pleasure of finding that the accelerating forces arising from the curvature in every point, were precisely such as would produce the accelerations necessary in those points for performing the motion that was required. And he exhibited the equations expressive of the state of the cord in all these points. And, on the faith of these equations, he restored the Taylorean curve to the rank which its inventor had given it; and he asserted that in every musical vibration the cord was disposed in a harmonical curve either simple or compound. He farther shewed that the equations which Euler and D'Alembert had given for the musical cord (at least in the cases which they had published) were included in his equations, and that their equations only exhibited its momentary states, while his own equations shewed the physical connection of them all; which is, that the whole cord forms a harmonic curve between the two fixed pins, while its different portions form subordinate harmonic curves on the first as an axis. Euler and D'Alembert, although they acknowledge this in the particular cases which they had taken as examples, on account of their simplicity, still insist that no subordinate harmonic vibrations can correspond to all the states of an elastic cord which their equations exhibit as isochronous and permanent. Mr. Bernoulli's death put an end to the controversy, and the question (considered as a general theory) is perhaps still undecided. It may very probably be true, that as a simple vibration may be permanent which never has the form of the simple harmonic described by Dr. Taylor, so a vibration may exist compounded of such vibrations, and therefore not expressible by any equation deduced from the Taylorean curve.

But, in the mean time, Mr. Bernoulli has made the most beautiful discovery in mechanics which has appeared in the

course of the last century, and has explained the most curious phenomenon of continued sounds, *viz.* the *almost universal* accompaniment of the harmonic notes of any fundamental sound. For this *susceptibility* of compounded variation is not confined to a 12th, but is equally demonstrable of every other harmonic. Nay, it is evident that the same simple vibration of a cord may furnish a moveable axis to more than one harmonic. For as the simple vibration can have a subordinate harmonic vibration superinduced upon it, so may this compounded vibration have another superinduced on it, and so on to any degree of composition. And farther, as Mr. Bernoulli has shewn the complete analogy between the accelerations of the different points of an elastic cord and of the corresponding plates of a column of air, it legitimately follows that all the consequences which we can easily deduce, respecting the vibrations of an elastic cord, may be affirmed respecting the undulations of a column of air in a pipe. Therefore this accompaniment of the harmonics must not be confined to the music of strings and bells, but equally obtains in the music of wind instruments. And thus the doctrine becomes universal.

Mr. Bernoulli did not think it enough to shew that these compound vibrations are possible. He endeavours to shew that this accompaniment must be frequent. He illustrates this very prettily, by supposing that a toothed wheel is turned round, and rubs with its teeth on an elastic cord. If the successive dropping of the teeth keep exactly pace with such vibrations as the cord can take and maintain by its elasticity, these will certainly be formed on it. If the intervals do not *exactly* correspond, a little reflection will shew that the agitation which the cord acquires will approximate to those which it can maintain; and, if when they are exactly so in any place of it, the wheel be in that instant removed, this vibration will remain and diffuse itself through the rest of the cord; so that the very last dying quiver (so to speak) will be a harmonic. Every *harmonic* agitation tends, by the

very nature of the thing, to continue, while those that are incompatible really *do* destroy each other; and the very last must be the remainder or superplus of such as could continue, over those which destroyed each other. Accordingly, the harmonic notes of wires are always most distinctly heard as the sound is dying away.

There is no occasion now to say any thing about the fallacy of Rameau's *Generation Harmonique* as a theory of musical pleasure. Our harmonies please us, not because a sound is accompanied by its harmonics, but because harmonics please. His principle is therefore a tautology, and gives no instruction whatever. His theory is a very *forced* accommodation of this principle to the practice of musicians, and taste of the public. He is exceedingly puzzled in the case of the *sous-dominante*, or 4th of the scale, and the 6th where there is no resonnance. He says that these notes, "fremissent, quoiqu'elles ne resonnent pas." But this misleads us. They do not resound; because a 4th and 6th cannot be produced at all by dividing the cord. They tremble; because the false 4th and false 6th are very near the true ones, and the true 4th and 6th would both tremble and resound, if they were made false. A string will both tremble and resound, if very nearly true, as any one observes the 12th and 17th on a harpsichord tremble and resound very strongly, though they are tempered notes. The whole theory is overturned at once by tuning the 4th false, so as to correspond to an aliquot division of the cord. It will then resound; and if this had happened to be agreeable, it would have been catched at as the sousdominante.

The physical cause of the pleasure of harmonic sounds is yet to seek, as much as our choice of those notes for melody which give us the best harmony (see TEMPERAMENT). We have no hesitation in saying that, with respect to our choice, the two are quite independent. Thousands enjoy the highest pleasure from melody who never heard a harmonious

sound. All the untaught singers, and all simple nations, are examples.

They not only fix on certain intervals as the steps of their tunes, but are disgusted when other steps are taken. Nor do we hesitate, for the very same reasons, to say that the rules of accompaniment are dependent on the cantus or air, and by no means on the fundamental bass of Rameau. The dependence assumed by him, as the rule of accompaniment, would, if properly adhered to, according to his own notions of the comparative values of the harmonics, lead to the most fantastic airs imaginable, always jumping by large intervals, and altogether incompatible with graceful music. The rules of modulation which he has squeezed out of his principle, are nothing but forced, very forced, accommodations of a very vague principle to the current practice of his contemporaries. They do not suit the primitive melodies of many nations, and they have caused these national musics to degenerate. This is acknowledged by all who are not perverted by the prevailing habits. We have heard, and could write down, some most enchanting lullabies of simple peasant women, possessed of musical sensibility, but far removed, in the cool sequestered vale of life, from all opportunities of stealing from our great composers. Some of these lullabies never fail to charm, even the most erudite musician, when sung by a fine flexible voice: but it would puzzle Mr. Rameau to accompany them *secundum artem.*

We conclude this subject by describing a most beautiful and instructive experiment.

Mr. Watt, the celebrated engineer, was amusing himself (about the year 1765) with organ-building, and invented a monochord of continued sound, by which he could tune an organ with mathematical precision, according to any proposed system of temperament. It consisted of a covered string of a violincello, sounding by the friction of an ivory wheel. The instrument did not answer Mr. Watt's purpose, by reason of the dead harshness of its tone, and a flutter in the

string by the unequal action of the wheel. But Mr. Watt was amused by observing the string frequently taking, of its own accord, points of division, which remain fixed, while the rest was in a state of strong vibration. The instrument came into the possession of the writer of this article. He soon saw that it gave him an opportunity of making all the experiments which Bernoulli could only relate. When the string was kept in a state of simple vibration, by a very uniform and gentle motion of the wheel, if its middle point was then gently touched with a quill, this point immediately stopped, but the string continued to vibrate in two parts, sounding the octave: And this it continued to do, however strong the vibrations were rendered afterwards by increasing the pressure and velocity of the wheel. The same thing happened if the string was gently touched at one third. It instantly divided itself into three parts, with two nodes, and sounded the 12th. In the same manner the double octave, the 17th, and all other harmonics, were produced and maintained.

But the prettiest experiment was to put something soft, such as a lock of cotton, in the way of the wide vibrations of the cord, at one-third and two-thirds of its length, so as to disturb them when they became very wide. When this was done, the string instantly put on the appearance of (Plate VII. fig. 8.) performing at once the full vibration competent to its whole length, and the three subordinate vibrations, corresponding to one-third of its length, and sounding the fundamental and the 12th with equal strength. In this manner all the different accompaniments were produced at pleasure, and could be continued, even with strong sounds. And it was amusing to observe, when the wheel was strongly pressed to the string, and the motion violent, the nodes would form themselves on various parts of the string, running from one part to another. This was always accompanied with all the jarring sounds which corresponded to them.

When the string was making very gentle, simple vibrations, and the wheel hardly touching it, if a violincello was made to sound the 12th very strongly in its neighbourhood, the string instantly divided itself, and vibrated in unison. frequently retaining its simple vibration and fundamental tone. We recommend this experiment to every person who wishes to make himself well acquainted with the mechanism of musical sounds. He will see, in a most sensible and convincing manner, how a single string of the Æolian harp gives us all the changes of harmony, sliding from one sound to another, according as it is affected in its different parts by an irregular breeze of wind. The writer of this article has attempted to regulate these sweet harmonic notes, and to introduce them into the organ. His success has been very encouraging, and the sounds far exceed in pathetic sweetness any that have yet been produced by that noble instrument. But he has not yet brought them fully under command, nor made them strong enough for any thing but the softest chamber music. Other necessary occupations prevent him from giving the attention to this subject that it deserves. He recommends it therefore to the musical instrument-makers as richly deserving their notice. His general method was this: A wooden pipe is made, whose section is a double square. A partition in the middle divides it into two pipes, along side of each other. One of them communicates with the foot and wind chest, and is shut at the upper end. The other is open at the upper, and shut at the lower end. In the partition there is a slit almost the whole length, and the sides of this slit are brought to a very smooth chamfered or feather edge. A fine catgut is strained in this slit, so as almost to touch the sides. It is evident that when the wind enters one pipe by the foot, it passes through the slit into the other, and escapes at the top, which is open. In its passage it forces the catgut into motion, and produces a musical note, having all the sweetness of the Æolian harp. The strength of sound may be increased by

increasing the body of air which is made to undulate. This was done by using, instead of catgut, very narrow silk tape or ribband varnished: but the unavoidable raggedness of the edges made the sounds coarse and wheezing. Flat silver wire was not sufficiently elastic; flat wire, used for watch balance springs, was better, but still very weak sounded. Other methods were tried, which promised better. A thin round plate of metal, properly supported by a spring, was set in a round hole; made in another plate not so thin, so as just not to touch the sides. The air forced through this hole made the spring plate tremble, dancing in and out, and produced a very bold and mellow sound.—This, and similar experiments, are highly worth attention, and promise great additions to our instrumental music.

WATCH-WORK.

Our intention in this article does not extend to the manual practice of this art, nor even to all the parts of the machine. We mean to consider the most important and difficult part of the construction, namely, the method of applying the maintaining power of the wheels to the regulator of the motion, so as not to hurt its power of regulation.

The regulator of a clock or watch is a pendulum or a balance. Without this check to the motion of the wheels, impelled by a weight or a spring, the machine would run down with a motion rapidly accelerating, till friction and the resistance of the air induced a sort of uniformity, as they do in a kitchen jack. But if a pendulum be so put in the way of this motion, that only one tooth of a wheel can pass it at each vibration, the revolution of the wheels will depend on the vibration of the pendulum. This has long been observed to have a certain constancy, insomuch that the astronomers of the East employed pendulums in measuring the times of their observations, patiently counting their vibra-

tions during the phases of an eclipse or the transits of the stars, and renewing them by a little push with the finger when they became too small. Gassendi, Riccioli, and others, in more recent times, followed this example. The celebrated physician Sanctorius is the first person who is mentioned as having applied them as regulators of clock movements. Machines, however, called *clocks*, with a train of toothed wheels leading round an index of hours, had been contrived long before. The earliest of which we have any account is that of Richard of Wallingford, Abbot of St. Alban's, in 1326 *. It appears to have been regulated by a fly like a kitchen jack. Not long after this Giacomo Dondi made one at Padau, which had a *motus succussorius*, a hobbling or trotting motion; from which expression it seems probable that it was regulated by some alternate movement. We cannot think that this was a pendulum, because, once it was introduced, it never could have been supplanted by a balance. The alternate motion of a pendulum, and its seeming uniformity, are among the most familiar observations of common life; and it is surprising that they were not more early thought of for regulating time measurers. The alternate motion of the old balance is one of the most far-fetched means that can be imagined, and might pass for the invention of a very reflecting mind, while a pendulum only requires to be drawn aside from the plumb-line to make it vibrate with regularity. The balance must be put in motion by the clock, and that motion must be stopped, and the contrary motion induced; and we must know that the same force and the same checks will produce uniform oscillations. All this must be previously known before

* Professor Beckmann, in the first volume of his *History of Inventions*, expresses a belief that clocks of this kind were used in some monasteries so early as the 11th century, and that they were derived to the monks from the Saracens. His authorities, however, are discordant, and seem not completely satisfactory even to himself.

we can think of it as a regulator; yet so it is that clocks, regulated by a balance, were long used, and very common through Europe, before Galileo proposed the pendulum, about the year 1600. Pendulum clocks then came into general use, and were found to be greatly preferable to balance clocks as accurate measurers of time. Mathematicians saw that their vibrations had some regular dependence on uniform gravity, and in their writings we meet with many attempts to determine the time and demonstrate the isochronism of the vibrations. It is amusing to read these attempts. We wonder at the awkwardness and insufficiency of the explanation given of the motions of pendulums, even by men of acknowledged eminence. Mersennus carried on a most useful correspondence with all the mathematicians of Europe, and was the means of making them acquainted with each other; nay, he was himself well conversant in the science; yet one cannot but smile at his reasonings on this subject. Standing on the shoulders of our predecessors, we look around us, in great satisfaction with our own powers of observation, not thinking how we are raised up, or that we are trading with the stock left us by the diligent and sagacious philosophers of the 17th century*. Riccioli, Gassendus, and Galileo, made similar attempts to explain the motion of pendulums; but without success. This honour was reserved for Mr. Huyghens, the most elegant of modern geometers. He had succeeded in 1656 or 1657 in adapting the machinery of a clock to the maintaining of the vibrations of a pendulum. Charmed with the accuracy of its performance, he began to investigate with scrupulous attention the theory of its motion. By the most ingenious and elegant application of geometry to mechanical problems, he demonstrated that the wider vibrations of a pendulum employed more time than the narrower, and that the time of a semicircular vibration is to that of a very small one nearly as 34 to 29; and, aided by a new department of geo-

metrical science invented by himself, namely, the evolution of curves, he shewed how to make a pendulum swing in a cycloid and that its vibrations in this curve are all performed in equal times, whatever be their extent.

But before this time, Dr. Hooke, the most ingenious and inventive mechanician of his age, had discovered the great accuracy of pendulum clocks, having found that the manner in which they had been employed had obscured their real merit. They had been made to vibrate in very large arches, the only motion that could be given them by the contrivances then known; and in 1656 he invented another method, and made a clock which moved with astonishing regularity. Using a heavy pendulum, and making it swing in very small arches, the clocks so constructed were found to excel Mr. Huyghen's cycloidal pendulums; and those who were unfriendly to Huyghens had a sort of triumph on the occasion. But this was the result of ignorance. Mr. Huyghens had shewn, that the error of $\frac{1}{100}$ of an inch, in the formation of the parts which produced the cycloidal motion, caused a greater irregularity of vibration than a circular vibration could do, although it should extend five or six degrees on each side of the perpendicular. It has been found that the unavoidable inaccuracies, even of the best artists, in the cycloidal construction, make the performance much inferior to that of a common pendulum vibrating in arches which do not exceed three or four degrees from the perpendicular. Such clocks alone are now made, and they exceed all expectation.

We have said that a pendulum needed only to be removed from the perpendicular, and then let go, in order to vibrate and measure time. Hence it might seem, that nothing is wanted but a machinery so connected with the pendulum as to keep a register, as it were, of the vibration. It could not be difficult to contrive a method of doing this; but more is wanted. The air must be displaced by the pendulum. This

requires some force, and must therefore employ some part of the momentum of the pendulum. The pivot on which it swings occasions friction—the thread, or thin piece of metal by which it is hung, in order to avoid this friction, occasions some expenditure of force by its want of perfect flexibility or elasticity. These, and other causes, make the vibrations grow more and more narrow by degrees, till at last the pendulum is brought to rest. We must therefore have a contrivance in the wheel-work which will restore to the pendulum the small portion of force which it loses in every vibration. The action of the wheels therefore may be called a *maintaining power*, because it keeps up the vibrations.

But we now see that this may affect the regularity of vibration. If it be supposed that the action of gravity renders all the vibrations isochronous, we must grant that the additional impulsion by the wheels will destroy that isochronism, unless it be so applied that the sum total of this impulsion and the force of gravity may vary so with the situation of the pendulum, as still to give a series of forces, or a law of variation, perfectly similar to that of gravity. This cannot be effected, unless we know both the law which regulates the action of gravity, producing isochronism of vibration, and the intensity of the force to be derived from the wheels in every situation of the pendulum.

The necessary requisite for the isochronous motion of the pendulum is, that the force which urges it toward the perpendicular, be proportional to its distance from it; and therefore, since pendulums swinging in small circular arches are sensibly isochronous, we must infer that such is the law by which the accelerating action of gravity on them is really accommodated to every situation in those arches.

It will greatly conduce to the better understanding of the effect of the maintaining power, if the reader keep in continual view the chief circumstances of a motion of this kind.

Therefore let AC *a* (Plate VIII. fig. 1.) represent the arch passed over by the pendulum, stretched out into a straight line. Let C be its middle point, when the pendulum hangs perpendicular, and A and *a* be the extremities of the oscillation. Let AD be drawn perpendicular to AC, to represent the accelerating action of gravity on the pendulum when it is at A. Draw the straight line DC *d*, and *a d*, perpendicular to A *a*. About C, as a centre, describe the semicircle AFH *a*. Through any points B, K, *k*, *b*, &c. of A *a*, draw the perpendiculars BFE, KLM, &c. cutting both the straight line and the semicircle. Then,

1. The actions of gravity on the pendulum, when in the situations B, K, &c. by which it is urged toward C, are proportional to, and may be represented by, the ordinates BE, KL, *b e*, *k l*, &c. to the straight line, DC *d*.

2. The velocities acquired at B, K, &c by the acceleration along AB, AK, &c. are proportional to the ordinates BF, KM, &c. to the semicircle AH *a*; and, therefore, the velocity with which the pendulum passes through the middle point C, is to its velocity in any other point B, as CH to BF.

3. The times of describing the parts AB, BK, KC, &c. of the whole arch of oscillation, are proportional to, and may be represented by, the arches AF, FM, MH, &c. of the semicircle.

4. If one pendulum describe the arch represented by AC *a*, and another describe the arch KC *k*, they will describe them in equal times, and their maximum velocities (viz. their velocities in the middle point,) are proportional to AC and KC; that is, the velocities in the middle point are proportional to the width of the oscillations.

The same proportions are true with respect to the motions outwards from C. That is, when the pendulum describes CA, with the initial velocity CH, its velocity at K is reduced to KM by the retarding action of gravity. It is

reduced to BF at B, and to nothing at A; and the times of describing CK, KB, BA, CA, are as HM, MF, FA, HA. Another pendulum setting out from C, with the initial velocity CO, reaches only to K, CK being = CO. Also the times are equal.—If we consider the whole oscillation as performed in the direction A a, the forces AD, BE, KL accelerate the pendulum, and the similar forces a d, b e, k l, on the other side, retard it. The contrary happens in the next oscillation aCA.

5. The areas DABE, DAKL, &c. are proportional to the squares of the velocities acquired by moving along AB, AK, &c. or to the diminution of the squares of the velocities sustained by moving outwards along BA or KA, &c.

The consideration of this figure will enable the reader (even though not a mathematician) to form some notion of the effect of any proposed application of a maintaining power by means of wheel-work: For, knowing the weight of the pendulum, we know the accelerating action of that weight in any particular situation A of the pendulum. We also know what addition or subtraction we produce on the pendulum in that situation by the wheel-work. Suppose it is an addition of pressure equal to a certain number of grains. We can make AD to D δ as the first to the last; and then A δ will be the whole force urging the pendulum toward C. Doing the same for every point of AC, we obtain a line $\delta \varepsilon \lambda$ C, which is a new scale of forces, and the space DC δ, comprehended between the two scales CD and C δ, will express the addition made to the square of the velocity by the maintaining power in passing along AC by the joint action of that power and the power of gravity. Also, by drawing a line $\varkappa \pi$ perpendicular to AC, making the space C $\pi \varkappa$ equal to CAD, the point π will be the limit of the oscillation outward from C, where the initial velocity HC is extinguished. If the line $\varkappa \pi$ cut the same circle in θ, one-half the arch θ A will nearly express the contraction made in the time of the outward oscillation by the maintaining power. An accurate determina-

tion of this last circumstance is operose, and even difficult; but this solution is not far from the truth, and will greatly assist our judgment of the effect of any proposal, even though $x\ \pi$ be drawn only by the judgment of the eye, making the area left out as nearly equal to the area taken in as we can estimate by inspection. This is said from experience.

Since the motion of a pendulum or balance is alternate, while the pressure of the wheels is constantly in one direction, it is plain that some art must be used to accommodate the one to the other. When a tooth of the wheel has given the balance a motion in one direction, it must quit it, that it may get an impulsion in the opposite direction. The balance or pendulum thus escaping from the tooth of the wheel, or the tooth escaping from the balance, has given to the general contrivance the name of SCAPEMENT among our artists, from the French word *echappement*. We proceed, therefore, to consider this subject more particularly, first considering the scapements which are peculiarly suited to the small vibrations of pendulums, and then those which must produce much wider vibrations in balances. This, with some other circumstances, render the scapements for pendulums and balances very different.

I. *Of the Action of a Wheel and Pallet.*

THE scapement which has been in use for clocks and watches ever since their first appearance in Europe, is extremely simple, and its mode of operation is too obvious to need much explanation. In Plate VIII. fig. 2. XY represents a horizontal axis, to which the pendulum P is attached by a slender rod, or otherwise. This axis has two leaves C and D attached to it, one near each end, and not in the same plane, but so that when the pendulum hangs perpendicularly, and at rest, the piece C spreads a few degrees to the right hand, and D as much to the left. They commonly make an angle of 70, 80, or 90 degrees. These two pieces are called PALLETS. AFB represents a wheel, turning

round on a perpendicular axis EO, in the order of the letters AFEB. The teeth of this wheel are cut into the form of the teeth of a saw, leaning forward, in the direction of the motion of the rim. As they somewhat resemble the points of an old-fashioned royal diadem, this wheel has got the name of the CROWN WHEEL. In watches it is often called the *balance wheel*. The number of teeth is generally odd; so that when one of them B is pressing on a pallet D, the opposite pallet C is in the space between two teeth A and I. The figure represents the pendulum at the extremity of its excursion to the right hand, the tooth A having just escaped from the pallet C, and the tooth B having just dropped on the pallet D. It is plain, that as the pendulum now moves over to the left, in the arch PG, the tooth B *continues* to press on the pallet D, and thus accelerates the pendulum, both during its descent along the arch PH, and its ascent along the arch HG. It is no less evident, that when the pallet D, by turning round the axis XY, raises its point above the plane of the wheel, the tooth B escapes from it, and I drops on the pallet C, which is now nearly perpendicular. I presses C to the right, and accelerates the motion of the pendulum along the arch GP. Nothing can be more obvious than this action of the wheel in maintaining the vibrations of the pendulum. We can easily perceive, also, that when the pendulum is hanging perpendicularly in the line XH, the tooth B, by pressing on the pallet D, will force the pendulum a little way to the left of the perpendicular, and will force it so much the farther as the pendulum is lighter; and, if it be sufficiently light, it will be forced so far from the perpendicular that the tooth B will escape, and then I will catch on C, and force the pendulum back to P, where the whole operation will be repeated. The same effect will be produced in a more remarkable degree, if the rod of the pendulum be continued through the axis XY, and a ball Q put on the other end to balance P. And, indeed, this is the contrivance which was first applied to clocks

all over Europe, before the application of the pendulum. They were balance clocks. The force of the wheel was of a certain magnitude, and therefore able, during its action on a pallet, to communicate a certain quantity of motion and velocity to the balls of the balance. When the tooth B escapes from the pallet D, the balls are then moving with a certain velocity and momentum. In this condition, the balance is checked by the tooth I catching on the pallet C. But it is not instantly stopped. It continues its motion a little to the left, and the pallet C forces the tooth I a little backward But it *cannot* force it so far as to escape over the top of the tooth I; because all the momentum of the balance was generated by the force of the tooth B; and the tooth I is equally powerful. Besides, when I catches on C, and C continues its motion to the left, its lower point applies to the face of the tooth I, which now acts on the balance by a long and powerful lever, and soon stops its farther motion in that direction, and now, continuing to press on C, it urges the balance in the opposite direction.

Thus we see that in a scapement of this kind, the motion of the wheel must be very hobbling and unequal, making a great step forward, and a short step backward, at every beat. This has occasioned the contrivance to get the name of the RECOILING SCAPEMENT, the recoiling pallets. This hobbling motion is very observable in the wheel of an alarm.

Thus have we obtained two principles of regulation. The first and most obvious, as well as the most perfect, is the natural isochronous vibration of a pendulum. The only use of the wheel-work here, besides registering the vibrations, is to give a gentle impulsion to the pendulum, by means of the pallet, in order to compensate friction, &c. and thus maintain the vibrations in their primitive magnitude. But there is no such native motion in a balance, to which the motion of the wheels must accommodate itself. The wheels, urged by a determined pressure, and acting through a determined space (the face of the pallet), must generate a

certain determined velocity in the balance; and therefore the time of the oscillation is also determined, both during the progressive and the retrograde motion of the wheel. The actions being similar, and through equal spaces, in every oscillation, they must employ the same time. Therefore a balance, moved in this manner, must be isochronous, and a regulator for a time-keeper.

By thus employing a balance, the horizontal position of the axis XY is unnecessary. Accordingly, the old clocks had this axis perpendicular, by which means the whole weight of the balance rested on the point of the pivot Y or X, according as the balance PQ was placed above or below. By making the supporting pivot of hard steel, and very sharp, friction was greatly diminished. Nay, it was entirely removed from this part of the machine by suspending the balance by a thread at the end X, instead of allowing it to rest on the point of the pivot Y.

As the balance regulator of the motion admits of every position of the machine, those clocks were made in an infinite variety of fanciful forms, especially in Germany, a country famous for mechanical contrivances. They were made of all sizes, from that of a great steeple clock, to that of an ornament for a lady's toilet. The substitution of a spring in place of a weight, as a first mover of the wheel-work, was a most ingenious thought. It was very gradual. We have seen, in the Emperor's museum at Brussels, an old, (perhaps the first) spring clock, the spring of which was an old sword blade, from the point of which a catgut was wound round the barrel of the first wheel. Some ingenious German substituted the spiral spring, which both took less room, and produced more revolutions of the first wheel.

When clocks had been reduced to such small sizes, the wish to make them portable was very natural; and the means of accomplishing this were obvious, namely, a farther reduction of their size. This was accomplished very early; and thus we obtained pocket watches, moved by a spiral spring,

and regulated by a balance with the recoiling scapement, which is still in use for common watches. The hobbling motion of the crown wheel is very easily seen in all of them.

It is very uncertain who first substituted a pendulum in place of the balance. Huyghens, as we have already observed, was the first who investigated the motions of pendulums with success, and his book *De Horologio Oscillatorio* may be considered as the elements of refined mechanics, and the source of all the improvements that have been made in the construction of scapements. But it is certain that Dr. Hooke had employed a pendulum for the regulation of a clock many years before the publication of the above-mentioned treatise, and he claims the merit of the invention of the *only proper* method of employing it. We imagine therefore that Dr. Hooke's invention was nothing more than a scapement for a pendulum making small vibrations, without making use of the opposite motions of the two sides of the crown wheel. Dr. Hooke had contrived some scapement more proper for pendulums than the recoiling pallets, because certainly those might be employed, and are actually employed as a scapement for pendulum clocks to this day, although they are indeed very ill adapted to the purpose. He had not only remarked the great superiority of such pendulum clocks as were made before Huyghens's publication of the cycloidal pendulum, over the balance clocks, but had also seen their defects, arising from the light pendulums and wide arches of vibration, and invented a scapement of the nature of those now employed. The pendulum clock which he made in 1658 for Dr. Wilkins, afterwards Bishop of Chester, is mentioned by the inventor, as peculiarly suited to the moderate swing of a pendulum; and he opposes this circumstance to a general practice of wide vibrations and trifling pendulums. The French are not in the practice of ascribing to us any thing that they can claim as their own; yet Lepaute says that the *Echappement à l'Ancre* came from England about the year 1665. It is also admitted by him

that clock-making flourished in England at that time, and that the French artists went to London to improve in it. Putting these and other circumstances together, we think it highly probable that we are indebted to Dr. Hooke for the scapement now in use. The principle of this is altogether different from the simple pallets and direct impulse already described; and is so far from being obvious, that the manner of action has been misunderstood, even by men of science, and writers of systems of mechanics.

In this scapement we employ those teeth of the wheel which are moving in one direction; whereas in the former scapement, opposite teeth were employed moving in contrary directions. Yet even here we must communicate an alternate motion to the axis of the pallets. The contrivance, in general, was as follows: On the axis A (See Plate VIII. fig. 3) of the pendulum or balance is fixed a piece of metal BAC, called the CRUTCH by our artists, and the ANCHOR by the French. It terminates in two faces B *b* C *c* of tempered steel, or of some hard stone. These are called the PALLETS, and it is on them that the teeth of the wheel act. The faces B *b* C *c* are set in such positions that the teeth push them out of the way. Thus B pushes the pallet to the left, and C pushes its pallet to the right. Both push their pallets sidewise outward from the centre of the wheel. The pallet B is usually called the *leading*, and C the *driving* pallet by the artists, although it appears to us that these names should be reversed, because B *drives* the pallet out of the way, and C *pulls* or leads it out of the way. They might be called the *first* and *second* pallet, in the order in which they are acted on by the wheel. We shall use either denomination. The figure is accommodated to the inactive or resting position of the pendulum. Suppose the pendulum drawn aside to the right at Q, and then let go. It is plain that the tooth B, pressing on the face of the pallet, β B *b* all the way from β to *b*, thrusts it aside outwards, and thus, by the connection of the crutch with the pendulum rod, aids the pendulum's mo-

tion along the arch QPR. When the pendulum reaches R, the point of the tooth B has reached the angle *b* of the pallet, and escapes from it. The wheel pressing forward, another tooth C drops on the pallet face C*c*, and, by pressing this pallet outward, evidently aids the pendulum in its motion from R to P. The tooth C escapes from this pallet at the angle *c*, and now a tooth B′ drops on the first pallet, and again aids the pendulum; and this operation is repeated continually.

The mechanism of this communication of motion is thus explained by several writers of elements. The tooth B (Plate VIII. fig. z.) is urged forward in the direction BD, perpendicular to the radius MB of the SWING WHEEL. It therefore presses on the pallet, which is moveable only in the direction BE, perpendicular to BA the radius of the pallet. Therefore the force BD must be resolved into two, *viz.* BE, in the direction in which alone the pallet can move, and ED, or BF, perpendicular to that direction. The last of these only presses the pallet and crutch against the pivot hole A. BE is the only useful force, or the force communicated to the pallet, enabling it to maintain the pendulum's motion, by restoring the momentum lost by friction and other causes.

But this is a very erroneous account of the *modus operandi*, as may be seen at once, by supposing the radius of the pallets to be a tangent to the wheel. This is a position most frequently given to them, and is the very position in Plate VIII. fig. 3. In this case MB is perpendicular to BA, and therefore BD will coincide with BA, and there will be no such force as BE to move the pendulum. It is a truth, deducible from what we know of the mechanical constitution of solid bodies, and confirmed by numberless observations, that when two solid bodies press on each other, either in impulsion or in dead pressure, the direction in which the mutual pressure is exerted is always perpendicular to the touching surfaces, whatever has been the direction of the

impelling body (See IMPULSION and MACHINERY.) Moreover this pressure is mutual, equal, and opposite. Whatever be the shapes of the faces of the tooth and pallet, we can draw a plane BN, which is the common tangent to both surfaces, and a line HBI through the point of contact perpendicular to BN. It is farther demonstrated in the article MACHINERY, that the action of the wheel on the pendulum is the same as if the whole crutch were annihilated, and in its stead there were two rigid lines AH, MI, from the centres of the crutch and wheel, perpendicular to HI, and connected by a third rigid line or rod HI, touching the two in H and I.

For if a weight V be hung at *v*, the extremity of the horizontal radius M *v* of the wheel, it will act on the lever *v* MI, pressing its point I upwards in the direction IH perpendicular to MI; the upper end of this rod IH will, in like manner, press the extremity H of the rod HA, and this will urge the pendulum from P toward R. To withstand this, the pendulum rod AP may be withheld by a weight *z*, hanging by a thread on the extremity of the horizontal lever A *z*, equal to M *v*, and connected with the crutch and pendulum. The weights V and *z* may be so proportioned to each other that, by acting perpendiculary on the crooked levers *v* MI, and *z* AH, the pressures at H and I shall be equal, and just balance each other by the intervention of the rod HI. When this is the case, we have put things into the same mechanical state, in respect of mutual action, as is effected by the crutch, pallets, and wheel, which, in like manner, produce equal pressures at B the point of contact, in the direction BH and BI. The weight V may be such as produces the very same effect at B that is produced by the previous train of wheel-work. The weight *z* therefore must be just equal to the force produced by the wheel-work on the point *z* of the pendulum rod, because by acting in the opposite direction it just balances it. Let us see there-

fore what force is communicated to the pendulum by the wheels.

Let x be the upward pressure excited at I, and y the equal opposite pressure excited at H. Then, by the property of the lever, we have $MI : M\,v = V : x$, and $x \times MI = V \times M\,v$. In like manner $y \times AH = Z \times A\,z$. Therefore, because $x = y$, and $A\,z = M\,v$, we have $V : Z = MI : AH$. That is, the force exerted by the tooth of the wheel in the direction of its motion is to the force impressed on the pendulum rod at a distance equal to the radius of the wheel as MI to AH. The force impressed on the ball of the pendulum is less than this in the proportion of AP to $A\,z$, or $M\,v$.

Cor. 1. If the perpendiculars MN, AO, be drawn on the tangent plane, the forces at B and z will be as BN to BO. For these lines are respectively equal to MI and AH.

Cor. 2. If HI meet the line of the centres AC in S, the forces will be as SM to SA; that is, $V : Z = SM : SA$.

Cor. 3. If the face β B b of the pallet be the evolutrix of a circle described with the radius AH, and the face of the tooth be the evolutrix of a circle described with the radius MI, the force impressed on the pendulum by the wheels will be constant during the whole vibration. But these are not the only forms which produce this constancy. The forms of teeth described by different authors, such as De la Hire, Camus, &c. for producing a constant force in trains of wheel-work, will have the same effect here. It is also easy to see that the force impressed on the pendulum may be varied according to any law, by making these faces of a proper form. Therefore the face, from B outwards, may be so formed that the force communicated to the pendulum by the wheels, during its descent from Q to P, may be in one constant proportion to the acceleration of gravity, and then the sum of the forces will be such as produces isochronous vibrations. If the inner part B b of the

face be formed on the same principle, the difference of the forces will have the same law of variation. If the face βb be the evolutrix of a circle, and the tooth B terminate in a point gently rounded, or quite angular, the force on the pendulum will continually increase as the tooth slides from β to b. For the line AH continues of the same magnitude, and MI diminishes. The contrary will happen, if the pallet be a point, either sharp or rounded, and if the face of the tooth be the evolutrix now mentioned; for MI will remain the same, while AH diminishes. If the tooth be pointed, and βb be a straight line, the force communicated to the pendulum will diminish, while the tooth slides from β to b. For in this case AH diminishes and MI increases.

Cor. 4. In general, the force on the pendulum is greater as the angle MB b increases, and as AB b diminishes.

Cor. 5. The angular velocity of the wheel is to that of the pendulum, in any part of its vibration, as AH to MI. This is evident, because the rod IH moving (in the moment under consideration) in its own direction, the points H and I move through equal spaces, and therefore the angles at A and M, must be inversely as the radii.

All that has now been said of the first pallet AB may be applied to the second pallet AC.

If the perpendicular C s be drawn to the touching plane o C n, cutting AM in s, we shall have $V : z = s\,M : s\,A$, as in Cor. 2. And if the perpendiculars Mi, A h, be drawn on C s, we have $V : Z = M\,i : A\,h$, as in the general theorem. The only difference between the action on the two pallets is, that if the faces of both are plain, the force on the pendulum increases during the whole of the action on the pallet C, whereas it diminishes during the progress of the tooth along the other pallet.

The reader will doubtless remark that each tooth of the wheel acts on both pallets in succession; and that, during its action on either of them, the pendulum makes one vibration. Therefore the number of vibrations during one turn

of the wheel is double the number of the teeth: consequently, while the tooth slides along one of the pallets, it advances half the space between two successive teeth; and when it escapes from the pallet, the other tooth *may* be just in contact with the other pallet. We say it *may* be so; in which case there will be no dropping of the teeth from pallet to pallet. This, however, requires very nice workmanship, and that every tooth be at precisely the same distance from its neighbour. Should the tooth which is just going to apply to a pallet chance to be a little too far advanced on the wheel, it would touch the pallet before the other had escaped. Thus, suppose that before B escapes from the point of the pallet, the tooth C is in contact with the pallet C and B cannot escape. Therefore when the pendulum returns from R towards Q, the pallet βb, returning along with it will push back the tooth B of the wheel. It does this in opposition to the force of the wheel. Therefore, whatever motion the wheel had communicated to the pendulum during its swing from P to Q, will now be taken from it again. The pendulum will not reach Q, because it had been aided in its motion from Q, and had proceeded further than it would have done without this help. Its motion toward Q is further diminished by the friction of the pallet. Therefore it will now return again from some nearer point q, and will not go so far as in the last vibration, but will return through a still shorter arch: And this will be still more contracted in the next vibration, &c. &c. Thus it appears that if a tooth chances to touch the pallet before the escape of the other, the wheel will advance no farther, and soon after the pendulum will be brought to rest.

For such reasons it is necessary to allow one tooth to escape *a little before* the other reaches the pallet on which it is to act, and to allow a small drop of the teeth from pallet to pallet. But it is accounted bad workmanship to let the drop be considerable, and *close scapement* is accounted a mark of care and of good workmanship. It is evidently an advan-

tage, because it gives a longer time of action on each pallet. This freeing the scapement cannot be accomplished by filing something from the face of the tooth; because this being done to all, the distance between them is diminished rather than augmented. The pallets must be first scaped as close as possible. This obliges the workman to be careful in making the teeth equidistant. Then a small matter is taken from the point of each pallet, by filing off the back $b\,r$ of the pallet. The tooth will now escape before it has moved through half a space.

From all that has been said on this particular, it appears that the interval between the pallets must comprehend a certain number of teeth, and half a space more.

The first circumstance to be considered in contriving a scapement is the angular motion that is intended to be given to the pendulum during the action of the wheel. This is usually called the *angle of scapement*, or the *angle of action*. Having fixed on an angle a that we think proper, we must secure it by the position and form of the face of the pallets. Knowing the number of teeth in the swing-wheel, divide 180° by this number, and the quotient is the angle b of the wheel's motion during one vibration of the pendulum. In the line AM, joining the centres of the crutch and wheel, make SM to SA, and s M to s A, as the angle a to the angle b; and then, having determined how many teeth shall be comprehended between the pallets, call this number n. Multiply the angle b by $2\,n + 1$, and take the half of the product. Set off this half in the circumference of the wheel (at the points of the teeth) on each side of the line joining the centres of the crutch and wheel, as at TB and TC. Through S and s draw SB and s C, and through B draw β B b perpendicular to SB, for the medium position of the face of the first pallet; that is, for its position when the pendulum hangs perpendicular. In like manner, drawing o C n perpendicular to s C, we have the medium position of the second pallet,

The demonstration of this construction is very evident fro what has been said.

We have hitherto supposed that the pendulum finishes i vibration at the instant that a tooth of the wheel escap from a pallet, and another tooth drops on the other palle But this is never, or should never be, the case. The pendu lum is made to swing somewhat beyond the angle of scap ment: for if it do not when the clock is clean and in goo order, but stop precisely at the drop of a tooth, then, whe it grows foul, and the vibration diminishes, the teeth will n escape at all, and the clock will immediately stop. Ther fore the force communicated by the wheels during the vibr tion within the limits of scapement, must be increased so to make the pendulum *throw* (as the artists term it) farth out; and a clock is more valued when it throws out conside ably beyond the angle of scapement. There are good re sons for this. The momentum of the pendulum, and i power to regulate the clock (which Mr. Harrison significan ly called its *dominion,)* is proportional to the width of i vibrations very nearly.

This circumstance of exceeding the angle of scapeme has a very great influence on the performance of the cloc or greatly affects the dominion of the pendulum. It is ea to see that, when the face $\beta\, b$ of the leading pallet is a plan if the pendulum continue its motion to the right, from P t ward Q, after the tooth B has dropped on it, the pallet wi push the wheel back again, while the tooth slides outwa on the pallet toward β. Such pallets therefore will make *recoiling scapement*, resembling, in this circumstance, the o pallet employed with the crown wheel, and will have th properties attached to this circumstance. One consequen of this is, that it is much affected by any inequalities of th maintaining power. It is a matter of the most familiar o servation, that a common watch goes slower when with a quarter of an hour of being down, when the action of th

spring is very weak, in consequence of its not pulling by a radius of the fusee. We observe the same thing in the beating of an alarum clock. Also if we at any time press forward the wheel-work of a common watch with the key, we observe its beats accelerate immediately. The reason of this is pretty plain. The balance, in consequence of the acceleration in the angle of scapement, would have gone much farther, employing a considerable time in the excursion. This is checked abruptly, which both shortens the vibration and the time employed in it. In the return of the pendulum, the motion is accelerated the whole way, along an arch which is shorter than what corresponds to its velocity in the middle point; for it is again checked on the other side, and does not make its full excursion. Moreover, all this irregularity of force, or the great deviation from a resistance to the excursion proportional to the distance from the middle point, is exerted on the pendulum when it is near the end of the excursion, where the velocity being small, this irregular force acts long upon it, at the very time that it has little force wherewith to resist it. All temporary inequalities of force, therefore, will be more felt in this situation of the balance than if they had been exerted in the middle of its motion. And although the regulating power of a pendulum greatly exceeds that of the light balances used in pocket watches, something of the same kind may be expected even in pendulum clocks. Accordingly this appears by a series of experiments made by Mr. Berthoud, a celebrated watch-maker of Paris. A clock, with a half second pendulum weighing five drams, was furnished with a recoiling scapement, whose pallets were planes. The angle of scapement was $5\frac{1}{2}$ degrees. When actuated with a weight of two pounds, it swung 8°, and lost 15″ per hour; with four pounds, it swung 10°, and lost 6″. Thus it appears that by doubling the maintaining power, although the vibration was increased in consequence of the greater impulse, the time was lessened 9″ per hour, viz. about $\frac{1}{400}$.

It is plain, from what was said when we described the first scapement, that an increase of maintaining power must render the vibration more frequent. We saw, on that occasion, that, even when the gravity of the pendulum is balanced by a weight on the other end of the rod, the force of the wheels will produce a vibratory motion, and that an augmentation of this force will increase it, or make the vibrations more rapid. The precise effect of any particular form of teeth can be learned only by computing the force on the pendulum in every position, and then constructing the curve $\delta \varepsilon \lambda$ C of Plate VIII fig. 1. The rapid increase of the ordinates beyond those of the triangle ADC, forms a considerable area DA π o, to compensate the area $\varkappa$ o C, and thus makes a considerable contraction A π of the vibration, and a sensible contraction $\frac{A \theta}{2}$ of the time.

Mr. George Grahame, the celebrated watchmaker in London, was also a good mathematician, and well qualified to consider this subject scientifically. He contrived a scapement, which he hoped would leave the pendulum almost in its natural state. The acting face of the pallet *a b c* (Plate VIII. fig. 4.) is a plane. The tooth drops on *a*, and escapes from *c*, and is on the middle point *b* when the pendulum is perpendicular. Beyond *a*, the face of the pallet is an arch *a d*, whose centre is A, the centre of the crutch. The maintaining power is made so great as to produce a much greater vibration than the angle of active scapement *a* A *c*. The consequence of this is that, when the tooth drops on the angle *a*, the pendulum, continuing its motion, carries the crutch along with it, and the tooth presses on the arch *a d*, in a direction passing through the centre of the crutch. This pressure can neither accelerate nor retard the motion of the crutch and pendulum. As the pendulum was accelerated after it passed the perpendicular, by the other pallet, it will (if quite unobstructed) throw out farther than what corresponds to the velocity which it had in the middle point

of its vibration; perhaps till the tooth passes from *a* to *e* on the circular arch of the pallet. But although it sustains no contrary action from the wheels during this excursion beyond the angle of scapement, it will not proceed so far, but will stop when the tooth reaches *d*; because there must be some resistance arising from the friction of the tooth along the arch *a d*, and from the clamminess of the oil employed to lubricate it: but this resistance is exceedingly minute, not amounting to $\frac{1}{8}$th of the pressure on the arch. Nay, we think that it appears from the experiments of Mr. Coulomb that, in the case of such minute pressures on a surface covered with oil, there is no sensible retardation analogous to that produced by friction, and that what retardation we observe arises entirely from the clamminess of the oil We are so imperfectly acquainted with the manner in which friction and viscidity obstruct the motions of bodies, that we cannnot pronounce decisively what will be their effect in the present case. Friction does not increase much, if at all, by an increase of velocity, and appears like a fixed quantity when the pressure is given. This makes all motions which are obstructed by friction terminate abruptly. This will shorten both the length and the time of the outward excursion of the pendulum. The viscidity of the oil resists differently, and more nearly in the proportion of the velocities. The diminution of motion will not be in this proportion, because in the greater velocities it acts for a shorter time. Were this accurately the case, the resistance of viscidity would also be nearly constant, and it would operate as friction does. But it does not stop a motion abruptly, and the motions are extinguished gradually. Therefore, although viscidity must always diminish the extent of the excursion, it may so vary as not to diminish the time. We apprehend, however, that it generally does. But whatever happens in the excursion, the return will certainly be slower, and employ more time than if it had not been obstructed, because the velocity in every point is less than if perfectly free. The whole arch,

consisting of a returning arch and an excursion on the other side, may be either slower or quicker, according as the compensation is complete or not, or is even overdone.

All these reflections occurred to Mr. Graham; and he was persuaded that the time of the tooth's remaining on the arch *a d*, both ascending and descending, would differ very little from that of the description of the same arch by a free pendulum. The great causes of irregularity seemed to be removed, viz. the inequalities in the action of the wheels in the vicinity of the extremity of the vibration, where the pendulum having little momentum is, long in the same little space, exposed to their action. The derangement produced by any force depends on the time of its action, and therefore must be greatest when the motion is slowest. The pendulum gets its impulse in the very middle of its vibration, where its velocity is the greatest; and therefore the inequalities of the maintaining power act on it only for a short time, and make a very trifling alteration in the time of its describing the arch of scapement. Beyond this, it is nearly in the state of a free pendulum; nay, even though it be affected by an inequality of the maintaining power, and it be accelerated beyond its usual rate in that arch, the chief effect of this will be to cause it to describe a larger arch of excursion. The shortening of the time of this description by the friction will be the same as before, happening at the very end of the excursion; but the return will be more retarded by the friction on a longer arch. And, by this, a compensation may be made for the trifling contraction of the time of describing the arch of scapement.

This circumstance of giving the impulse in the middle of the vibration, where its time of action is the smallest possible, and whereby the pendulum is so long left free from the action of the wheels, is of the very first importance in all scapements, and should ever be in the mind of the mechanician. When this is adhered to, the form of the face *a b c* is scarcely of any moment. Much has been written on this

form, and many attempts have been made to make it such that the action of the wheel shall be proportional to the action of gravity. To do this is absolutely impossible. Mr. Graham made them planes, not only because of easiest execution, but because a plane really conspires pretty well with the change of gravity. While the pendulum moves from Q to P (Plate VIII. fig. 3.), the force of gravity, acting in the direction QP, is continually diminishing. So is the accelerating power of the pallet from *a* to *b*. When the pendulum rises from P to R, a force in the opposite direction RP continually increases. This is analogous to the continual diminution of a force in the direction PR. Now we have such a diminution of such a force, in the action of the pallet from *b* to *c*, and such an augmentation in the action of the other pallet.

For all these reasons, this construction of a scapement appeared very promising. Mr. Graham put it in practice, and it answered his most sanguine expectation, and is now universally adopted in all nice clocks. Mr. Graham, however, did not think it prudent to cause a tooth to drop on the very angle *a* of the pallet. He made it drop on a point *f* of the arch of excursion. This has also the advantage of diminishing the angle of action, which we have proved to be of service. It requires, indeed, a greater maintaining power; but this can easily be procured, and is less affected by the changes to which it is liable by the effect of heat and cold on the oil. Our observations on the effects of friction and viscidity in the arch *a d* seem to be confirmed by the observations of several artists, who agree in saying that a great increase of maintaining power increases the vibrations, but makes them perceptibly slower. When they wrote, much oil was applied to diminish the friction on the arch of repose; but, since that time, the rubbing parts were made such as required no oil, and this retardation disappeared. In the clock of the transit room of the Royal Observatory, the angle of action seldom exceeds one-third of the swing of

the pendulum. The pallets are of oriental ruby, and the wheel is of steel tempered to the utmost degree of hardness. This clock never varies a whole second from equable motion in the course of five days.

This contrivance is known by the name of the DEAD BEAT, the DEAD SCAPEMENT; because the seconds index stands still after each drop, whereas the index of a clock with a recoiling scapement is always in motion, hobbling backward and forward.

These scapements, both recoiling and dead beat, have been made in a thousand forms; but any person tolerably acquainted with mechanics, will see that they are all on the same principles, and differ only in shape or some equally unimportant circumstance. Perhaps the most convenient of any is that represented in (Plate VIII. fig. 5.) where the shaded part is the crutch, made of brass or iron, and A and B are two pieces of agate, flint, or other hard stone, cut into the proper shape for a pallet of either kind, and firmly fixed in proper sockets, They project half an inch, or thereabouts, in front of the crutch, so that the swing wheel is also before the crutch, distant about $\frac{1}{10}$th of an inch or so. Pallets of ruby, driven by a hard steel swing wheel, need no oil, but merely to be once rubbed clean with an oily cloth.

Sometimes the wheel has pins instead of teeth. They are ranged round the rim of the wheel, perpendicular to its plane, and both pallets are on one side of the wheel, standing perpendicular to its plane. One of these pins drops from the first to the second pallet at once. The pallets are placed on two arms, as in Plate VIII. fig. 6. in which case the pins are alternately on different sides of the wheel; or on one, as in Plate VIII. fig. 7. By the motion of the pendulum to the right, the pin (in Plate VIII. fig. 7.), after resting on the concave arch *d a*, acts on the face *a c*, and drops from *c* on the other concave arch *i g*, which continues to move a little way to the right. It then returns, and the pin slides and

acts on the pallet *i h*, and escapes at *h*; and the next pin is then on the arch of repose *d a*.

It being evident that the recoiling scapement accelerates the vibrations beyond the rate of a free pendulum, and it also appearing to many of the first artists that the dead scapement retards them, they have attempted to form a scapement which shall avoid both of these defects, by forming the arches *a d*, *i g*, so as to produce a very small recoil. Mr. Berthoud does this in a very simple manner, by placing the centre of *a d* at a small distance from that of the crutch, so as to make the rise of the pallet above the concentric arch about one-third of the arch itself. Applying such a crutch to the light pendulum mentioned in a former paragraph, he found that doubling, and even trebling the maintaining power, produced no change in the time of vibration, though it increased the width from 8° to 12° and 14°. We have no doubt of the efficacy of this contrivance, and think it very proper for all clocks which require much oil, such as turret clocks, &c. But we apprehend that no rule can be given for the angle that the recoiling arch should make with the concentric one. We imagine that this depends entirely on the share which friction and oil have in producing the retardation of the dead beat.

Other artists have endeavoured to avoid the inconveniences of friction and oil on the arch of repose in another way. Instead of allowing the tooth of the wheel to drop on the back of the pallet, which we call the *arch of excursion*, and others call the *arch of repose*, it drops on a detent *o t a* (Plate VIII. fig. 8.), of which the part *t a* is part of an arch whose centre is A, the centre of the crutch, and the part *t o* is in the direction of the radius. This piece does not adhere to the pallet, but is on the end of an arm *o* A, which turns round the axis A of the crutch on fine pivots: it is made to apply itself to the back of the pallet by means of a slender spring A *p*, attached to the pallet, and pressing inward on a pin *p*, fixed in the arm of the detent. When so applied, its arch

t a makes the repose, and its point *a* makes a small portion of the face *a c* of the pallet.

The action of this apparatus is very easily understood. When a tooth escapes from the second pallet, by the motion of the pendulum from the left to the right, another tooth drops on this pallet (which the figure shews to be the first or leading pallet) at the angle *t*, and rests on the small portion *t a* of an arch of repose. But the crutch, continuing its motion to the right, immediately quits the arm *o* A, carrying the pallet *a c r* along with it, and leaving the wheel locked on the detent *o t a*. By and bye the pendulum finishes its excursion to the right, and returns. When it enters the arch of action, the pallet has applied itself to the detent *o t a*, and withdraws it from the tooth. The tooth immediately acts on the face *a c* of the pallet, and restores the motion lost during the last vibration. The use of the spring is merely to keep the detent applied to the pallet without shaking. It is a little bent during their separation, and adds something of an opposing force to the ascent of the pendulum on the other side of the wheel, and accelerates its return. A similar detent on the back of the second pallet performs a similar office, supporting the wheel while the pendulum is beyond the arch of scapement, and quitting it when the pendulum enters that arch.

We do not know who first practised this very ingenious and promising invention. Mr. Mudge certainly did so early as 1753 or 1754. Mr. Berthoud speaks obscurely of contrivances of the same nature. So does Le Roy, and (we think) Le Paute. We say that it is very promising. Friction is almost annihilated by transferring it to the pivots at A; so that, in the excursion beyond the angle of scapement, the pendulum seems almost free. Indeed some artists of our acquaintance have even avoided the friction of the pivots at A, by making the arm of the detent a spring of considerable thickness, except very near to A, where it is made very thin and broad. But we do not find that this construction,

though easily executed, and susceptible of great precision and steadiness of action, is much practised. We presume that the performance has not answered expectations. It has not been superior to the incomparably more simple dead scapement of Graham. Indeed we think that it cannot. A part of the friction still remains, which cannot be removed; namely, while the arch *t a* is drawn from between the tooth and pallet. Nay, we apprehend that something more than friction must be overcome here. The tooth is apt to force the detent outward, unless the part *t a* be a little elevated at its point *a* like a claw, above the concentric arch, and the face of the tooth be made to incline forward, so as to fit this shape of the detent. This will consume some force, when the momentum of the pendulum is by no means at its maximum. Should the clock be foul, and the excursions beyond scapement be very small, this disturbance must be exceedingly pernicious. But we have a much greater objection. During the whole excursion beyond scapement, there is a new force of a spring acting on the pendulum, which deviates considerably from the proportions of the accelerating power of gravity. It does not commence its action till the detent separates from the arm of the crutch. Then the spring of the detent acts as a retarding force against the excursion of the pendulum, now on the other side, bringing it sooner to rest, and then accelerating it in its way back to the beginning of the arch of scapement. In short, this construction should have the properties of a recoiling scapement. We got a clock-maker to make some experiments on one which he had made for an amateur, which fully confirmed our conjecture. When the detent spring was strong, an increase of maintaining power made the vibrations both wider and more rapid. The artist reduced the strength of the spring till this effect was rendered very small. It might perhaps be quite removed by means of a still weaker spring: But the spring was already so weak, that a hard step on the floor of the room did sometimes disengage the detent from the

wheel. It appears, therefore, that nothing can be reasonably expected from this construction that is not as well performed by the dead scapement of Mr. Graham, of much easier execution and more certain performance.

Very similar to this construction (at least in the excursion beyond the angle of scapement) is the construction of Mr. Cumming, and it has the same defects. His pallets are carried, as in the one described, by the crutch. The detents press on them behind by their weight only: therefore, when the tooth is locked on the detent of one pallet, its weight is taken off from the pendulum on that side, and the weight of the detent on the other side opposes the ascent, and accelerates the descent of the pendulum.

Mr. Cumming executed another scapement, consisting, like those, of a pallet and detent. But the manner of applying the maintaining power is extremely different in principle from any yet described. It is exceedingly ingenious, and seems to do all that is possible for removing every source of irregularity in the maintaining power, and every obstruction to free motion arising from friction and oil in the scapement. For this reason we shall give such an account of its essential circumstances as may suffice to give a clear conception of its manner of acting, and its good properties and defects; but referring the inquisitive reader to Mr Cumming's Elements of Clock and Watch-Work, published in 1766, for a more full account.

In the scapements last described, the pallets were fixed to the crutch and pendulum, and the maintaining power, during its action, was applied to the pendulum by means of the pallets, in the same way as in ordinary scapements. The detents were unconnected with the pendulum, and it was free during the whole excursion. In the present scapement both the pallets and detents are detached from the pendulum, except in the moment of unlocking the wheel; so that the pendulum may be said to be free during its whole vibration, except during this short moment.

ABC (Plate VIII. fig. 9.) represents a portion of the swing wheel, of which O is the centre, and A one of the teeth; Z is the centre of the crutch, pallets, and pendulum. The crutch or detents is represented of a form resembling the letter A, having in the circular cross piece a slit *i k*, also circular, Z being the centre. This form is very different from Mr. Cumming's, and inferior to his, but was adopted here in order to avoid a long description. The arm ZF forms the first detent, and the tooth A is represented as locked on it at F. D is the first pallet on the end of the arm Z *d* moveable round the same centre with the detents; but moveable independently of them. The arm *d e*, to which the pallet D is attached, lies altogether behind the arm ZF of the detent, being fixed to a round piece of brass *e f g*, which has pivots turning concentric with the verge or axis of the pendulum. To the same round piece of brass is fixed the horizontal arm *e* H, carrying at its extremity the ball H, of such size, that the action of the tooth A on the pallet D is just able (but without any risk of failing) to raise it up to the position here drawn. Z P *p* represents the fork, or the pendulum rod, behind both detent and pallet. A pin *p* projects forward, coming through the slit *i k*, without touching the upper or under margin of it. There is also attached to the fork the arm *m n* (and a similar one on the other side,) of such length that, when the pendulum rod is perpendicular, as is represented here, the angular distance of *n q* from the rod *e q* H is precisely equal to the angular distance of the left side of the pin *p* from the left end *i* of the slit *i k*.

The mode of action on this apparatus is abundantly simple. The natural position of the pallet D is at ∂, represented by the dotted lines, resting on the back of the detent F. It is naturally brought into this position by its own weight, and still more by the weight of the ball H. The pallet D, being set on the fore side of the arm at Z, comes into the same plane with the detent F and the swing-wheel. It is

drawn, however, in the figure in another position. The tooth C of the wheel is supposed to have escaped from the second pallet, on which the tooth A immediately engages with the pallet D, situated at δ, forces it out, and then rests on the detent F, the pallet D leaning on the tip of the tooth. F is brought into this situation in a way that will appear presently. After the escape of C, the pendulum, moving down the arch of semivibration, is represented as having attained the vertical position. Proceeding still to the left, the pin *p* reaches the extremity *i* of the slit *i k*; and, at the same instant, the arm *n* touches the rod *e* H in *q*. The pendulum proceeding a hair's breadth further, withdraws the detent F from the tooth, which now even pushes off the detent, by acting on the slant face of it. The wheel being now unlocked, the tooth following C on the other side acts on its pallet, pushes it off, and rests on its detent, which has been rapidly brought into a proper position by the action of A on the slant face of F. It was a similar action of C on its detent in the moment of escape, which brought F into a fit position for locking the wheel by the tooth A. The pendulum still going on, the arm *m n* carries the weight of the ball H and the pallet connected with it, and it comes to rest before the pin *p* again reaches the end of the slit, which had been suddenly withdrawn from it by the action of A on the slant face of F. The pendulum now returns towards the right loaded on the left with the ball H, which restores the motion which it had lost during the last vibration. When, by its motion to the right, the pin *p* reaches the end *k* of the slit *i k*, it unlocks the wheel on the right side. At the same instant the weight H ceases to act on the pendulum, being now raised up from it by the action of a tooth like B on the pallet D.

Let us now consider the mechanism of these motions. The prominent feature of the contrivance is the almost complete disengagement of the regulator from the wheels. The wheels, indeed, act on the pallets; but the pallets are then

detached from the pendulum. The sole use of the wheel is to raise the little weights while the pendulum is on the other side, in order to have them in readiness at the arrival of the pendulum. They are then laid on the pendulum, and supply an accelerating force, which restores to the pendulum the momentum lost during the preceding vibration. Therefore no inequalities in the action of the wheel on the pallets, whether arising from friction or oil, has any effect on the maintaining power. It remains always the same, namely, the rotative momentum of the two weights. The only circumstance, in which the irregularity of the action of the wheels can affect the pendulum is at the moment of unlocking. Here indeed the regulator may be affected; but this moment is so short, in comparison with other scapements, that it must be considered as a real improvement.

It is very uncandid to refuse the author a claim to the character of an ingenious artist on account of this contrivance, as has been done by a very ingenious university Professor, who taxes Mr. Cumming with ignorance of the *first elements of mechanics*, and says that the best thing in his book is his advice to suspend the pendulum from a great block of marble, firmly fixed in the wall. This is certainly a good advice, and we doubt not but that the Professor's clock would have performed still better if he had condescended to follow it. It is still less candid to question the originality of the invention. We know for certain that it was invented at a time and place where the author *could* not know what had been done by others. It would have been more like the urbanity of a well-educated man to have acknowledged the genius, which, without similar advantages, had done so much.

But, while we thus pay the tribute of justice to Mr. Cumming, we do not adopt all his opinions. The clock has the same defects as the former in respect of the law of the force which accelerates the pendulum. The sudden addition of the small weight, and this almost at the extremity of the vi-

bration, would derange it very much, if the addition were susceptible of any sensible variation. The irregularity of the action of the wheels *may* sensibly affect the motion during the unlocking, when the clock is foul, and the pendulum *just* able to unlock; for any disturbance at the extremity of the vibration greatly affects the time. We acknowledge that the parts which we here suppose to be foul may not be so in the course of twenty years, these parts being only the pivots of the scapement. The great defect of the scapement is its liableness to unlock by any jolt. It is more subject to this than the others already mentioned. This risk is much increased by the slender make of the parts, in Mr. Cumming's drawings, and in the only clock of the kind we have seen; but this is not necessary: and it should be avoided for another reason; the interposing so many slender and crooked parts between the moving power and the pendulum weakens the communication of power, and requires a much more powerful wheel-work.

All these, however, are slight defects, and only the last can be called a fault. The clocks made on this principle have gone remarkably well, as may be seen by the registers of his majesty's private observatory. But the greatest objection is, that they do not perform better than a well-made dead scapement; and they are vastly more troublesome to make and to manage. This is strictly true, and is a serious objection. The fact is, that the dominion of a heavy pendulum is so great, that if *any one* of the scapements now described be well executed with pallets of agate, and a wheel of hard steel, and if the pendulum be suspended agreeably to Mr. Cumming's advice, there is hardly any difference to be observed in their performance. We shall content ourselves with a single proof of this from fact. The clock invented by the celebrated Harrison is *at least equal* in its performance to any other. Friction is almost annihilated, and no oil is required. It went fourteen years without being touched, and during that time did not vary one complete

second from one day to another, nor ever deviated half a minute by accumulation from equable motion. Yet the scapement, in so far as it respects the law of the accelerating force, deviates more from the proportion of the spaces than the most recoiling scapement that ever was put to a good clock. It is so different from all hitherto described, both in form and principle, that we must not omit some account of it, and with it we shall conclude our scapements for clocks.

Let GDO (Plate VIII. fig. 10.) represent the swing-wheel, of which M is the centre. A is the verge or axis of the pendulum. It has two very short arms AB, AE. A slender rod BC turns on fine pivots in the joint B, and has at its extremity C a hook or claw, which takes hold of a tooth D of the swing-wheel when the pendulum moves from the right side to the left. This claw, when at liberty, stands at right angles, or, at least, in a certain determinate angle, with regard to the arm AB; and when drawn a little from that position, it is brought back to it again by a very slender spring. The arm AE is furnished with a detent EF, which also, when at liberty, maintains its position on the arm by means of a very slender spring.

Let us now suppose that the tooth D is pressing on the claw C, while the pendulum is moving to the right. The joint B yields, by its motion round A, to the pressure of the tooth on the claw. By this yielding, the angle ABC opens a little. In the mean time, the same motion round A causes the point F of the detent on the other side to approach the circumference of the wheel in the arch of a circle, and the tooth G at the same time advances. They meet, and the point of G is lodged in the notch under the projecting heel *f*. When this takes place, it is evident that any farther motion of the point E round A must push the tooth G a little backwards, by means of the detent EF. It cannot come any nearer to the wheel, because the point of the tooth stops the heel *f*. The instant that F pushes G back, the

tooth D is withdrawn from the claw C, and C flies out, by the action of its spring, and resumes its position at right angles to BA; and the wheel is now free from the claw, but is pushing at the detent F *. The pendulum, having finished its excursion to the right (in which it causes the wheel to recoil by means of the detent F,) returns toward the left. The wheel now advances again, and, by pressing on F, aids the pendulum through the whole angle of scapement. By this motion the claw C describes an arch of a circle round A, and approaches the wheel, till it take hold of another tooth, namely, the one following D, and pulls it back a little. This immediately frees the detent F from the pressure of the tooth G, and it flies out a little from the wheel, resuming its natural position by means of its spring. Soon after, the motion of the pendulum to the left ceases, and the pendulum returns; D pulling forward the hook C to aid the pendulum, and the former operation is repeated, &c. &c.

Such is the operation of the pallets of Harrison and Hindley. Friction is almost totally avoided, and oil entirely †.

* The reader may here remark the manner in which the pressure of the tooth G on the detent is transferred to the joint E by the intervention of the shank FE, and from the joint E to the pendulum rod, by the intervention of the arm EA. This communication of pressure is precisely the same that we made use of in explaining the common scapement. MG, FE, and EA, in this fig. 10. are performing the offices which we then gave to the lines MI, BH, and HA, in fig. 3. Harrison's pallet realises the abstract theory.

† Mr. Harrison was at first by profession a carpenter in a country place. Being extremely ingenious and inventive, he had made a variety of curious wooden clocks. He made one, in particular, for a turret in a gentleman's house. Its exposure made it waste oil very fast, and the maker was often obliged to walk two or three miles to renew it, and got nothing for his trouble. In trudging home, not in very good humour, he pondered with himself how to make a clock go without oil. He changed all his pinion leaves into rollers; which answered very well. But the pallets required it more than any other part. After various other projects, he contrived those now represented, where there was no friction, and no oil is wanted. The turret clock continued to go without being touched till Mr. Harrison left the country.

The motion is given to the pendulum by a fair pull or push, and the teeth of the wheel only apply themselves to the detents without rubbing. There is no drop, and the scapement makes no noise, and is what the artists calls *a silent scapement*. The mechanician will readily perceive, that by properly disposing the arms AB, AE, and disposing the pallets on the circumference of the wheel, the law, by which the action of the wheel on the pendulum is regulated, may be greatly varied, so as to harmonize, as far as the nature of scapement, alternately pushing and pulling, will admit, with the action of gravity.

But this is evidently a recoiling scapement, and one of the worst kind; for the recoil is made at the very confines of the vibration, where every disturbance of the regular cycloidal vibration occasions the greatest disturbance to the motion. Yet this clock kept time with most unexampled precision, far excelling all that had been made before, and equal to any that have been made since. This is entirely owing to the immense superiority of the momentum of the pendulum over the maintaining power.

II. *Of Scapements for a Watch.*

The execution of a proper scapement for watches is a far more delicate and difficult problem than the foregoing, on account of the small size, which requires much more accurate workmanship, because the error of the hundredth part of an inch has as great a proportion to the dimensions of the regulator as an inch in a common house clock. It is much more difficult on another account. We have no such means of accumulating such a dominion (to use Mr. Harrison's expressive term) over the wheel-work in the regulator of a watch as in that of a clock. The heaviest balance that we can employ, without the certainty of snapping its pivots by every slight jolt, is a mere trifle, in comparison with the pendulum of the most ordinary clock. A dozen or twenty

grains is the utmost weight of the balance, even of a very large pocket watch. The only way that we can accumulate any notable quantity of regulating power in such a small pittance of matter, is by giving it a very great velocity. This we do by accumulating all its weight in the rim, by giving it very wide vibrations, and by making them extremely frequent. The balance-rim of a middling good watch should pass through at least ten inches in every second. Now, when we reflect on the small momentum of this regulator, the inevitable inequalities of the maintaining power, and the great arch of vibration on which these inequalities will operate, and the comparative magnitude even of an almost insensible friction or clamminess, it appears almost chimerical to expect any thing near to equability in the vibrations, and incredible that a watch can be made which will not vary more than one beat in 86400. Yet such have been made. They must be considered as the most masterly exertions of human art. The performance of a reflecting telescope is a great wonder: the worst that can find a market must have its mirrors executed without an error of the ten-thousandth part of an inch; but we now know that this accuracy is attained almost in spite of us, and that we scarcely *can* make them of a worse figure. But the case is far otherwise in watch-work. Here all those wonderful approaches to perfection are the results of rational discussion, by means of sound principles of science; and, unless the artist who puts these principles into practice be more than a mere copyist, unless the principles themselves are perceived by him, and actually direct his hand, the watch may still be good for nothing. Surely, then, this is a liberal art, and far above a manual knack. The study of the means by which such wonders are steadily effected, is therefore the study of a gentleman.

In the account given above of the scapements for pendulums, we assumed, as one leading principle, that *the natural vibrations of a pendulum are performed in equal times, whether wide or narrow.* This is so nearly true, when the arches on

each side of the perpendicular do not exceed four degrees, that the retardation of the wider arches within that limit will not become sensible, though accumulated for a long time. The common scapement with a plane face of the pallet, helps to correct even this small inequality much better than the nicest form of the cycloidal cheeks proposed by Huyghens.

In watch-work we assume a similar principle, namely, that *the oscillations of a balance, urged by a spring, and undisturbed by all foreign forces, are performed in equal times, whether they be wide or narrow.* This principle was assumed by the celebrated mechanician Dr. Robert Hooke, on the authority of many experiments which he had made on the bending and unbending of springs. He found that the force necessary for retaining a spring in any constrained position was proportional to its tension, or deflection from its natural form. He expressed this in an anagram, which he published about the year 1660, in order to establish his claim to the discovery, and yet conceal it, till he had made some important application of it. When the anagram was explained some years afterwards, it was, "*Ut tensio, sic vis.*" Dr. Hooke thought of applying this discovery to the regulation of watch movements. For, if a slender spring be *properly* applied to the axis of a watch balance, it will put that balance in a certain determinate position. If the balance be turned aside from this position, it seems to follow that it will be urged back towards it by a force proportional to its distance from it. He immediately made the application to an old watch, which he afterwards gave to Dr. Wilkins, Bishop of Chester. This was in 1658. Its motion was so amazingly improved, that Hooke was persuaded of the perfection of his principle, and thought that nothing was now wanting for making a watch of this kind a perfect chronometer but the hand of a good workman. For his watch seemed almost perfect, though made in a small country town, in a very coarse manner. Mr. Huyghens also claims

this discovery. He published his claim about the year 1675, and proposed to make watches for discovering the longitude of a ship at sea. But there is the most unquestionable evidence of Dr. Hooke's priority by fifteen years, and of his having made several watches of this kind. One of them was in the possession of his majesty king Charles II. Dr. Hooke's first balance spring was straight, and acted on the balance in a very imperfect manner. But he soon saw the imperfections, and made several successive alterations; and, among others, he employed the cylindrical spiral now employed by Mr. Arnold; but he gave it up for the flat spiral; and the king's watch had one of this kind before Mr. Huyghens published his invention. His project of longitude watches had been carried on along with Lord Brouncker and Sir Robert Moray, and they had quarreled some years before that publication.

But both Dr. Hooke and Mr. Huyghens were too sanguine in their expectations. We, by no means, have the evidence for the truth of this principle that we have for the accelerating action of gravity on a pendulum. It rests on the nicety, and the propriety of the experiments; and long experience has shewn that it is sensibly true only within certain limits. The demonstrations by which Bernoulli supports the unqualified principle of Mr. Huyghens, proceed on hypothetical doctrines concerning the nature of elasticity. And even these shew that the law of elasticity which he assumed was selected, not because founded on simpler principles than any other, but because it was consistent with the experiments of Hooke and Huyghens. Besides, although this should be the true law of a spring, it does not follow that this spring, applied in *any way* to the axis of a balance, will urge that balance agreeably to the same law; and if it did, it still does not follow that the oscillations of the balance will be isochronous; for the force has to move not only the balance, but also the spring. Part of the restoring force of the spring is employed in restoring it rapidly to its quiescent

shape, and thus enabling it to *follow and still impel* the yielding balance. It is therefore only the surplus which is employed in actually moving the balance, and it is uncertain whether this surplus varies according to the same law, being always the same proportion of the whole force of the spring. We find it an extremely difficult problem to determine the law of variation of this surplus, even in the simplest form of the spring; nay, it is by no means an easy problem to determine the law of oscillation of a spring, unloaded with any balance; and we can easily shew that there are such forms of a spring, that although the velocity with which the different parts approach to their quiescent position be exactly as their excursion from it, this is by no means the law of velocity which this spring will produce in a balance. The matter of fact is, that when the spring is a simple straight steel wire, suspending the balance in the direction of its axis, the motions of it, if not immoderate, are precisely agreeable to Huyghens's and Hooke's rule; and that the motion of a balance urged by a spring wound up into a flat, or a cylindrical spiral, as in common watches, and those of Arnold, deviates sensibly from it, unless a certain analogy be preserved between the length and the elasticity of the spring. If the spring be immoderately long, the wide vibrations are slower than the narrow ones; and the contrary is observed when the spring is immoderately short. A certain taper, or gradual diminution of the spring, is also found to have an effect in equalizing the wide and narrow vibrations. There is also a great difference between the force with which a part of the spring unbends itself, and the action of that force in urging the balance round its axis; and the performance of many watches, good in other respects, is often faulty from the manner in which this unbending force is employed.

But, since these corrections are in our power in a considerable degree, we may suppose them applied, and the true motion (which we shall call the cycloidal) attained; and we may then adapt the construction of the scapement to the pre-

serving this motion undisturbed. And here we must see at once that the problem is incomparably more delicate than in the case of pendulums. The vibrations must be very wide, and the angular motion rapid, that it may be little affected by external motions. The smallest inequalities of maintaining power acting through so great a space, must bear a considerable proportion to the very minute momentum of a watch balance. Oil is as clammy on the pallets of a watch as on those of a clock; a viscidity which would never be felt by a pendulum of 20 pounds weight will stop a balance of 20 grains altogether. For the same reason, it is evident that any impropriety in the form of the pallet must be incomparably more pernicious than in the case of a pendulum; the deviation which this may occasion from a force proportional to the angular distance from the middle point, must bear a great proportion to the whole force.

The common recoiling scapement of the old clocks still holds its place in the ordinary pocket watches, and answers all the common purposes of a watch very well. A well finished watch, with a recoiling scapement, will keep time within a minute in the day. This is enough for the ordinary affairs of life. But such watches are subject to great variation in their rate of going, by any change in the power of the wheels. This is evident; for if the watch be held back, or pressed forward, by the key applied to the fusee square, we hear the beating greatly retarded or accelerated. The maintaining power, in the best of such watches, is never less than one fifth of the regulating power of the spring. For, if we take off the balance spring, and allow the balance to vibrate by the impulse of the wheels alone, we shall find the minute hand to go forward from 25 to 30 minutes per hour. Suppose it 30. Then, since the wheels act through equal spaces with or without a spring, the forces are as the squares of the acquired velocities. The velocity in this case is double; therefore the accelerating force is quadruple, and the force of the spring is three times that of the wheels. If

the hand goes forward 25 minutes, the force of the wheels is about one-fifth of that of the spring This great proportion is necessary, as already observed, that the watch may go as soon as unstopped.

We have but little to say on this scapement; its principle and manner of action, and its good and bad qualities, being the same with those of the similar scapement for pendulums. It is evident that the maintaining power being applied in the most direct manner, and during the whole of the vibration, it will have the greatest possible influence to move the balance. A given mainspring and train will keep in motion a heavier balance by means of this scapement than by any other. But, on the other hand, and for the same reason, the balance has less dominion over the wheel-work, and its vibrations are more affected by any irregularities of the wheel-work. Moreover, the chief action of the wheel being at the very extremities of the vibrations, and being very abrupt, the variations in its force are most hurtful to the isochronism of the vibrations.

Although this scapement is extremely simple, it is susceptible of more degrees of goodness or imperfection than almost any other, by the variation of the few particulars of its construction. We shall therefore briefly describe that construction which long experience has sanctioned as approaching near to the best performance that can be obtained from the common scapement. Plate IX. fig. 1. represents it in what are thought its best proportions, as it appears when looking straight down on the end of the balance arbor. C is the centre of the balance and verge. CA and CB are the two pallets; CA being the upper pallet, or the one next to the balance, and CB being the lower one. F and D are two teeth of the crown wheel, moving from left to right; and E, G, are two teeth on the lower part of the circumference, moving from right to left. The tooth D is represented as just escaped from the point of CA, and the lower tooth E as just come in contact with the lower pallet. The scapement

should not, however, be quite so close, because an inequality on the teeth might prevent D from escaping at all. For if E touch the pallet CB before D has quitted CA, all will stand still. This fault will be corrected by withdrawing the wheel a little from the verge, or by shortening the pallets.

The proportions are as follow. The distance between the front of the teeth (that is, of G, F, E, D) and the axis C of the balance is one-fifth of FA, the distance between the points of the teeth. The length CA, CB of the pallets is three-fifths of the same distance. The pallets make an angle ACB of 95 degrees, and the front DH or FK of the teeth make an angle of 25° with the axis of the crown-wheel. The sloping side of the tooth must be of an epicycloidal form, suited to the relative motion of the tooth and pallet.

From these proportions it appears that the pallet A can *throw out*, by the action of the tooth D, till it reaches *a*, 120 degrees from CL, the line of the crown-wheel axis. For it can throw out till the pallet B strike against the front of E, which is inclined 25° to CL. To this add BCA, = 95°, and we have LC $a = 120$. In like manner B will throw out as far on the other side. From 240°, the sum of these angles, take the angle of the pallets 95°, and there remains 145 for the greatest vibration which the balance can make without striking the front of the teeth. This extent of vibration supposes the teeth to terminate in points, and the acting surfaces of the pallets to be planes directed to the very axis of the verge. But the points of the teeth must be rounded off a little for strength, and to diminish friction on the face of the pallets. This diminishes the angle of scapement very considerably, by shortening the teeth. Moreover, we must by no means allow the point of the pallet to bank or strike on the foreside of a tooth. This would greatly derange the vibration by the violence and abruptness of the check which the wheel would give to the

pallet. This circumstance makes it improper to continue the vibrations much beyond the angle of scapement. One-third of a circle, or 120°, is therefore reckoned a very proper vibration for a scapement made in these proportions. The impulse of the wheels, or the angle of scapement, may be increased by making the face of the pallets a little concave (preserving the same angle at the centre). The vibration may also be widened by pushing the wheel nearer to the verge. This would also diminish the recoil. Indeed this may be entirely removed by bringing the front of the wheel up to C, and making the face of the pallet not a radius, but parallel to a radius and behind it, *i. e.* by placing the pallet CA so that its acting face may be where its back is just now. In this case, the tooth D would drop on it at the centre, and lie there at rest, while the balance completes its vibration. But this would make the banking (as the stroke is called) on the teeth almost unavoidable. In short, after varying every circumstance in every possible manner, the best makers have settled on a scapement very nearly such as we have described. Precise rules can scarcely be given; because the law by which the force acting on the pallets varies in its intensity, deviates so widely from the action of the balance spring, especially near the limits of the excursions.

The discoveries of Huyghens and Newton in rational mechanics engaged all the mathematical philosophers of Europe in the solution of mechanical problems, about the end of the last century. The vibrations of elastic plates or wires, and their influence on watch balances, became familiar to every body. The great requisites for producing isochronous vibrations were well understood, and the artists were prompted by the speculatists to attempt constructions of scapements proper for this purpose. It appeared clearly, that the most effectual means for this purpose was to leave the balance unconnected with the wheels, especially near the extremities of the vibration, where the motion is languid, and where every inequality of maintaining power must act

for a longer time, and therefore have a great effect on the whole duration of the vibrations. The maxim of construction that naturally arises from these reflections, is to *confine*, if possible, *the action of the wheels to the middle of the vibration*, where the motion is rapid, and where the chief effect of an increase or diminution of the maintaining power will be to enlarge or contract the angular motions, but will make little change on their duration; because the greatest part of the motion will be effected by the balance spring alone. This maxim was inculcated in express terms by John Bernoulli, in his *Recherches Mechaniques et Physiques;* but it had been suggested by common sense to several unlettered artists before that time. About the beginning of this century watches were made in London, where the verge had a portion *e d b* (Plate IX. fig. 2.) of a small cylinder, having its centre *c* in the axis, and a radial pallet *b a* proceeding from it. Suppose a tooth just escaped from the point of the pallet, moving in the direction *b d e*, the cylindrical part was so situated that the next tooth dropped on it at a small distance from its termination. While the verge continues turning in the direction *b d e*, the tooth continues resting on the cylinder, and the balance sustains no *action* from the wheels, and has only to overcome the minute frictions on the polished surface of a hard steel cylinder. This motion may perhaps continue till the pallet acquires the position *f*, almost touching the tooth. It then stops, its motion being extinguished by the increasing force of the spring. It now returns, moving in the direction *e d b*; and when the pallet has acquired the position *c i*, the tooth *g* quits the circumference of the cylinder, and drops in on the pallet at the very centre. The crooked form of the tooth allows the pallet to proceed still farther, before there is any danger of banking on the tooth. This vibration being also ended, the balance resumes its first direction, and the tooth now acts on the face of the pallet, and restores to the balance all the motion which

it had lost by friction, &c. during the two preceding vibrations.

It is evident that this construction obviates all the objections to the former recoiling scapement, and that, by sufficiently diminishing the diameter of the cylindrical part, the friction may be reduced to a very small quantity, and the balance be made to move by the action of the spring during the whole of the excursion, and of the returning vibration. Yet this construction does not seem to have come much into use, owing, in all probability, to the great difficulty of making the drop so accurate in all the teeth. The smallest inequality in the length of a tooth would occasion it to drop sooner or later; and if the cylinder was made very small, to diminish friction, the formation of the notch was almost a microscopical operation, and the smallest shake in the axis of the verge or the balance-wheel would make the tooth slip past the cylinder, and the watch run down amain.

About the same time, a French artist in London (then the school of this art) formed another scapement, with the same views. We have not any distinct account of it, but are only informed (in the 7th volume of the *Machines approuvées par l'Acad. des Sciences*) that the tooth rested on the surface of a hollow cylinder, and then escaped by acting on the inclined edge of it. But we may presume that it had merit, being there told that Sir Isaac Newton wore a watch of this kind.

A much superior scapement, on the same principle, was invented by Mr. Geo. Graham, at the same time that he changed the recoiling scapement for pendulums into the dead beat. Indeed it is the same scapement, accommodated to the large vibrations of a balance. In Plate IX. fig. 3. DE represents part of the rim of the balance-wheel. A and C are two of its teeth, having their faces *b c* formed into planes, inclined to the circumference of the wheel, in an angle of about 15 degrees; so that the length *b c* of the face is nearly quadruple of its height *e m*. Suppose a cir-

cular arch ABC described round the centre of the wheel, and through the middle of the faces of the teeth. The axis of the balance passes through some point B of this arch, and we may say that the mean circumference of the teeth passes through the centre of the verge. On this axis is fixed a portion of a thin hollow cylinder *b c d*, made of hard tempered steel, or of some hard and tough stone, such as ruby or sapphire. Agates, though very hard, are brittle. Chalcedony and cornelian are tough, but inferior in hardness. This cylinder is so placed on the verge, that when the balance is in its quiescent position, the two edges *b* and *d* are in the circumference which passes through the points of the teeth. By this construction the portion of the cylinder will occupy 210° of the circumference, or 30° more than a semicircle. The edge *b*, to which the tooth approaches from without, is rounded off on both angles. The other edge *d* is formed into a plane, inclined to the radius about 30°.

Now, suppose the wheel pressed forward in the direction AC. The point *b* of the tooth, touching the rounded edge, will push it outwards, turning the balance round in the direction *b c d*. The heel *e* of the tooth will escape from this edge when it is in the position *h*, and *e* is in the position *f*. The point *b* of the tooth is now at *d*, but the edge of the cylinder has now got to *i*. The tooth, therefore, rests on the inside of the cylinder, while the balance continues its vibration a little way, in consequence of the shove which it has received from the action of the inclined plane pushing it out of the way, as the mould-board of a plough shoves a stone aside. When this vibration is ended, by the opposition of the balance-spring, the balance returns, the tooth (now in the position B) rubbing all the while on the inside of the cylinder. The balance comes back into its natural position *b c d*, with an accelerated motion, by the action of its spring, and would of itself, vibrate as far, at least, on the other side. But it is aided again by the tooth, which, pressing on the edge *d*, pushes it aside, till it come into the position *k*, when the

tooth escapes from the cylinder altogether. At this moment the other edge of the cylinder is in the position *l*, and therefore is in the way of the next tooth, now in the position A. The balance continues its vibration, the tooth all the while resting, and rubbing on the outside of the cylinder. When this vibration, in the direction *d c b*, is finished, the balance resumes its first motion *b c d*, by the action of the spring, and the tooth begins to act on the first edge *b*, as soon as the balance gets into its natural position, shoves it aside, escapes from it, and drops on the inside of the cylinder. In this manner are the vibrations produced, gradually increased to their maximum, and maintained in that state. Every succeeding tooth of the wheel acts first on the edge *b*, and then on the edge *d*; resting first on the outside, and then on the inside of the cylinder. The balance is under the influence of the wheels while the edge *b* passes to *h*, and while *d* passes to *k*; and the rest of the vibration is performed without any *action* on the part of the wheels, but is a little obstructed by friction, and by the clamminess of the oil. In the construction now described, the arch of action or scapement is evidently 30°, being twice the angle which the face of a tooth makes with the circumference.

The reader will perceive, that when this scapement is executed in such a manner that the succeeding tooth is in contact with the cylinder at the instant that the preceding one escapes from it, the face of the tooth must be equal to the inside diameter of the cylinder, and that the distance between the heel of one tooth and the point of the following one must be equal to the outside diameter. When the scapement is so close there is no drop. A good artist approaches as near to this adjustment as possible; because, while a tooth is dropping, but not yet in contact, it is not acting on the balance, and some force is lost. The execution is accounted very good, if the distance between the centres of two teeth is twice the external diameter of the cylinder. This allows

a drop equal to the thickness of the cylinder, which is about $\frac{1}{20}$th of its diameter.

We must also explain how this cylinder is so connected with the verge as to make such a great revolution round the tooth of the wheel. The triangular tooth *e b m* is placed on the top of a little pillar or pin fixed into the extremity of the piece of brass *m* D formed on the rim of the wheel. Thus the wedge-tooth has its plane parallel to the plane of the wheel, but at a small distance above. Plate IX. fig. 10 represents the verge, a long hollow cylinder of hard steel. A great portion of the metal is cut out. If it were spread out flat, it would have the shape of Plate IX. fig. 12. Suppose this rolled up till the edges GH and G′H′ are joined, and we have the exact form. The part acted on by the point of the tooth is the dotted line *b d*. The part DIFE′ serves to connect the two ends. Thus it appears to be a very slender and delicate piece; but being of tempered steel, it is strong enough to resist moderate jolts. The ruby cylinders are much more delicate.

Such is the cylinder scapement of Mr. Graham, called also the HORIZONTAL SCAPEMENT, because the balance wheel is parallel to the others. Let us see how far it may be expected to answer the intended purposes. If the excursions of the balance beyond the angle of impulsion were made altogether unconnected with the wheels, the *whole vibration* would be quicker than one of the same extent, made by the action of the balance-spring alone, because the middle part of it is accelerated by the wheels. But the excursions are obstructed by friction and the clamminess of oil. The effect of this in *obstructing* the motion is very considerable. Mr. Le Roy placed the balance so, that it rested when the point of the tooth was on the middle of the cylindric surface. When the wheel was allowed to press on it, and it was drawn 80° from this position, it vibrated only during $4\frac{1}{2}$ seconds. When the wheel was not allowed to touch the cy-

linder, it vibrated 90 seconds, or 20 times as long; so much did the friction on the cylinder exceed that of the pivots. We are not sufficiently acquainted with the laws of either of these obstructions to pronounce decidedly whether they will increase or diminish the *time* of the whole vibrations. We observe distinctly, in motions with considerable friction, that it does not increase nearly so fast as the velocity of the motion; nay, it is often less when the velocity is very great. In all cases it is observed to terminate motions abruptly. The friction requires a certain force to overcome it, and if the body has any less it will stop. Now this will not only contract the excursion of the balance, but will shorten the time. But the return to the angle of impulsion will undoubtedly be of longer duration than the excursion; for the arch of return, from the extremity of the excursion to its beginning, where the angle of impulsion ends, is the same with the arch of excursion. The velocity which the balance has in any point of the return is less than what it had in the same point of the excursion; because, in the excursion, it had velocity enough to carry it to the extremity, and also to overcome the friction. In the return, it could, even without friction, only have the velocity which would have carried it to the extremity; and this smaller velocity is diminished by friction during the return. The velocity being less through the whole return than during the excursion, the time must be greater. It may therefore happen that this retardation of the return may compensate the contraction of the excursion and the diminution of its duration. In this case the vibration will occupy the same time as if the balance had been free from the wheels. But it may more than compensate, and the vibrations will then be slower; or it may not fully compensate, and they will be quicker. We cannot therefore say, *à priori*, which of the two will happen: but we may venture to say that an increase of the force of the wheels will make the watch go slower: for this will exert a greater pressure, give a greater impulsion, pro-

duce a wider excursion, and increase the friction during that greater excursion, making the wide vibrations slower than the narrow ones; because the angle of impulsion remaining the same, the pressures exerted must be quadrupled, in order to double the excursion, and therefore the friction will be increased in a greater proportion than the momentum which is to overcome it. But, with respect to the obstruction arising from the viscidity of the oil, we know that it follows a very different law. It bears a manifest relation to the velocity, and is nearly proportional to it. But still it is difficult to say how this will affect the whole vibration. The duration of the excursion will not be so much contracted as by an equal obstruction from friction, because it will not terminate the motion abruptly. There are therefore more chances of the increased duration of the return exceeding the diminution of it in the excursion. All that we can say, therefore, is, that there will be a compensation in both cases. The time of excursion will be contracted, and that of return augmented.

Now, as the friction may be greatly diminished by fine polish, fine oil, and a small diameter of the cylinder, we may reasonably expect that the vibrations of such a balance will not vary nearly so much from isochronism as with a recoiling scapement, and will be little affected by changes in the force of the wheels. Accordingly, Graham's cylindrical scapement supplanted all others as soon as it was generally known. We cannot compare the vibrations with those of a free balance, because we have no way of making a free balance vibrate for some hours. But we find that doubling or trebling the force of the wheels makes very little alteration in the rate of the watch, though it greatly enlarges the angular motion. Any one may perceive the immense superiority of this scapement over the common recoiling scapement, by pressing forward the movement of a horizontal watch with the key, or by keeping it back. No great change can be observed in the frequency of the beats, however hard

we press. But a more careful examination shews that an increase of the power of the wheels generally causes the watch to go slower; and that this is more remarkable as the watch has been long going without being cleaned. This shews that the cause is to be ascribed to the friction and oil operating on the wide arches of excursion. But when this scapement is well executed, in the best proportions of the parts, the performance is extremely good. We know such watches, which have continued for several weeks without ever varying more than 7″ in one day from equable motion. We have seen one whose cylinder was not concentric with the balance, but so placed on the verge that the axis of the verge was at *o* (Plate IX. fig. 3.), between the centre B of the cylinder and the entering edge *b*, and B*e* was equal to the thickness of the cylinder. The watch was made by Emery of London, and was said to go with astonishing regularity, so as to equal any time-piece while the temperature of the air did not vary; and when clean, was said to be less affected by the temperature than a watch with a free scapement, but unprovided with a compensation piece. It is evident that this watch must have a minute recoil. This was said to be the aim of the artist, in order to compensate for the obstruction caused by friction during the return of the balance from its excursions. It indeed promises to have this effect; but we should fear that it subjects the excursions to the influence of the wheels. We suspect that the indifferent performance of cylinder watches may often arise from the cylinder being off the centre in some disadvantageous manner.

The watch from which the proportions here stated were taken, is a very fine one made by Graham for Archibald Duke of Argyle, which has kept time with the regularity now mentioned. We believe that there are but few watches which have so large a portion of the cylinder: few indeed have more than one half, or 180° of the circumference. But this is too little. The tooth of the wheel does not begin to

act on the resting cylinder till its middle point A or B touch one of the edges. To obtain the same angle of scapement, the inclination of the face of the tooth must be increased (it must be doubled); and this requires the maintaining power to be increased in the same proportion. Besides, in such a scapement it may happen that the tooth will never rest on the cylinder; because the instant that it quits one edge it falls on the other, and pushes it aside, so that the balance acquires no wider vibration than the angle of scapement, and is continually under the influence of the wheels. The scapement is in its best state when the portion of the cylinder exceeds 180° by twice the inclination of the teeth to the circumference of the wheel.

It would employ volumes to describe all the scapements which have been contrived by different artists, aiming at the same points which Graham had in view. We shall only take notice of such as have some essential difference in principle.

Plate IX. fig. 4. represents a scapement invented in France, and called the *Echappement à* VIRGULE, because the pallet resembles a comma. The teeth A, B, C, of the balance wheel are set very oblique to the radius, and there is formed on the point of each a pin, standing up perpendicular to the plane of the wheel. This greatly resembles the wheel of Graham's scapement, when the triangular wedge is cut off from the top of the pin on which it stands. The axis *c* of the verge is placed in the circumference passing through the pins. The pallet is a plate of hard steel *a e f d b*, having its plane parallel to the plane of the wheel. The inner edge of this plate is formed into a concave cylindrical surface between *o* and *b*, whose axis *c* coincides with the axis of the verge. Adjoining to this is the acting face *b d* of the pallet. This is either a straight line *b d*, making an angle of nearly 30° with a line *c b g* drawn from the centre, or it is more generally curved, according to the nostrum of the artist. The back of the pallet *a e f* is also a cylindrical sur-

face (convex) concentric with the other. This extends about 100° from *a* to *f*. The part between *f* and *d* may have any shape. The interval *a o* is formed into a convex surface, in such a manner as to be every where intersected by the radius in an angle of 30° nearly; *i. e.* it is a portion of an equiangular spiral. The whole of this is connected with the verge by a crank, which passes perpendicularly through it between *f* and *e*; and the plate is set at such height on the crank or verge, that it can turn round clear of the wheel, but not clear of the pins. The teeth of the wheel are set so obliquely, and made so slender, that the verge may turn almost quite round without the crank's banking on the teeth. The part *f d b*, called the horn, is of such a length, that when one pin B rests on the outside cylinder at *a*, the point *d* is just clear of the next pin A.

When the wheel is not acting, and the balance spring is in equilibrio, the position of the balance is such that the point *d* of the horn is near *i*, about 30° from *d*. The figure represents it in the position which it has when the tooth A has just escaped from the point *d* of the horn. In this position the next tooth B is applied to the convex cylinder, a very little way (about 5°) from its extremity *a*. This description will enable the reader to understand the operation of the virgule scapement.

Now suppose the pin A just escaped from the horn. The succeeding pin B is now in contact with the back of the cylinder; and the balance, having got an impulse by the action of A along the concave pallet *b d*, continues its motion in the direction *d g h*, till its force is spent, the point of the horn arriving perhaps at *h*, more than 90° from *d*. All this while the following tooth B is resting on the back *e f* of the cylinder. The balance now returns, by the action of its spring; and when the horn is at *i*, the pin gets over the edge *a o*, and drops on the opposite side of the concave cylinder, where it rests, while the horn moves from *i* to *k*, where it stops, the force of the balance being again spent.

The balance then returns; and when the horn comes within 30° of *d*, the pin gets out of the hollow cylinder, shoves the horn out of its way, and escapes at *d*. Besides the impulse which the balance receives by the action of the wheel on the horn *b d*, there is another, though smaller, action in the contrary direction, while the point of B passes over the surface *a o*; for this surface being inclined to the radius, the pressure on it urges the balance round in the direction *h d i*.

The chief difference of this scapement from the former is that the inclined plane is taken from the teeth of the wheel, and placed on the verge. This alone is a considerable improvement; for it is difficult to shape all the teeth alike; whereas the horn *b d* is invariable. Moreover, the resting parts, although they be drawn large in this figure for the sake of distinctness, may be made vastly smaller than Graham's cylinder, which must be big enough to hold a tooth within it. By this change, the friction, during the repose of the wheel, that is, during the excursions of the balance, may be vastly diminished. The inside cylinder need be no bigger than to receive the pin. But although the performance of these scapements is excellent, they have not come into general use in this country. The cause seems to be the great nicety requisite in making the pins of the wheel pass exactly through the axis of the verge. The least shake in the pivots of the balance and balance-wheel must greatly change the action. A very minute increase of distance between the pivots will cause the pin B to slide from the edge *a* to the horn, without resting at all on the inside cylinder; and when it does so, it will stop the balance at once, and, immediately after, the watch will run down. The same irregularities will happen if all the pins be not at precisely the same distance from the axis of the wheel.

This scapement was greatly improved, and, in appearance, totally changed, by Mr. Lepaute of Paris in 1753. By placing the pins alternately on thetwo sides of the rim

of the balance-wheel, he avoided the use of the outside cylinder altogether. The scapement is of such a singular form, that it is not easy to represent it by any drawing. We shall endeavour, however, to describe it in such a manner as that our readers, who are not artists, will understand its manner of acting. Artists by profession will easily comprehend how the parts may be united which we represent as separate.

Let ABC (Plate IX. fig. 5.) represent part of the rim of the balance-wheel, having the pins 1, 2, 3, 4, 5, &c. projecting from its faces; the pins, 1, 3, 5, being on the side next the eye, but the pins 2 and 4 on the farther side. D is the centre of the balance and verge, and the small circle round D represents its thickness. But the verge in this place is crooked, like a crank, that the rim of the wheel may not be interrupted by it. This will be more particularly described by and bye. There is attached to it a piece of hard tempered steel *a b c d*, of which the part *a b c* is a concave arch of a circle, having D for its centre. It wants about 30° of a semicircle. The rest of it *c d* is also an arch of a circle, having the same radius with the balance-wheel. The natural position of the balance is such, that a line drawn from D, through the middle of the face *c d*, is a tangent to the circumference of the wheel. But, suppose the balance turned round till the point *d* of the horn comes to *d'*, and the point *c* comes to 2, in the circumference in which the pins are placed. Then the pin, pressing on the beginning of the horn or pallet, pushes it aside, slides along it, and escapes at *d*, after having generated a certain velocity in the balance. So far this scapement is like the virgule scapement described already. But now let another pallet, similar to the one now described, be placed on the other side of the wheel, but in a contrary position, with the acting face of the pallet turned away from the centre of the wheel. Let it be so placed at E, that the moment that the pin 1, on the upper side of the wheel, escapes from the pallet *c d*, the pin

4, on the under side of the wheel, falls on the end of the circular arch *e f g* of the other pallet. Let the two pallets be connected by means of equal pulleys G and F on the axis of each, and a thread round both, so that they shall turn one way. The balance on the axis D, having gotten an impulse from the action of the pin 1, will continue its motion from A towards *i*, and will carry the other pallet with a similar motion round the centre E from *h* towards *k*. The pin 4 will therefore rest on the concave arch *g f e* as the pallet turns round. When the force of the balance is spent, the pallet *c d* returns towards its first position. The pallet *g h* turns along with it; and when the point of the first has arrived at *d*, the beginning *g* of the other arrives at the pin 4; and, proceeding a little farther, this pin escapes from the concave arch *e f g*, and slides along the pallet *g h*, pushing it aside, and therefore urging the pallet round the centre E, and consequently (by means of the connection of the pulleys) urging the balance on the axis D round at the same time, and in the same direction. The pin 4 escapes from the pallet *g h*, when *h* arrives at 3; but in the time that the pin 4 was sliding along the yielding pallet *g h*, the pin 3 is moving in the circumference BDA; and the instant that the pin 4 escapes from *h* at 3, the pin 3 arrives at 2, and finds the beginning *c* of the concave arch *c b a* ready to receive it. It therefore rests on this arch, while the balance continues its motion. This perhaps continues till the point *b* of the arch comes to 2. The balance now stops, its force being spent, and then returns; and the pin 3 escapes from the circle at *c*, slides along the yielding pallet *c d*, and when it escapes at 1, another pin on the under side of the wheel arrives at 4, and finds the arch *g f e* ready to receive it. And in this manner will the vibration of the balance be continued.

This description of the mode of action at the same time points out the dimensions which must be given to the parts of the pallet. The length of the pallet *c d* or *g h* must be

equal to the interval between two succeeding pins, and the distance of the centres D and E must be double of this. The radius D *e* or E *g* may be as small as we please. The concave arches *c b a* and *g f e* must be continued far enough to keep a pin resting on them during the whole excursion of the balance. The angle of scapement, in which the balance is under the influence of the wheels, is had by drawing D *c* and D *d*. This angle *c* D *d* is about 30°, but may be made greater or less.

Plate IX. Fig. 11. will give some notion how the two pallets may be combined on one verge. KL represents the verge with a pivot at each end. It is bent into a crank MNO, to admit the balance wheel between its branches, BC represents this wheel seen edgewise, with its pins, alternately on different sides. The pallets are also represented edgewise by *b c d* and *h g f*, fixed to the inside of the branches of the crank, fronting each other. The position of their acting faces may be seen in the preceding figure, on the verge D, where the pallet *gh* is represented by the dotted line 2 *i*, as being situated behind the pallet *c d*. The remote pallet 2 *i* is placed so, that when the point *d* of the near pallet is just quitted by a pin 1 on the upper side of the wheel, the angle formed by the face and the arch of rest of the other pallet is just ready to receive the next pin 2, which lies on the under side of the rim. A little attention will make it plain, that the action will be precisely the same as when the pallets were on separate axes. The pin 1 escapes from *d*, and the pin 2 is received on the arch of rest, and locks the wheel while the balance is continuing its motion. When it returns, 2 gets off the arch of rest, pushes aside the pallet 2 *i*, escapes from it when *i* gets to 1, and then the pin 3 finds the point *c* ready to to receive it, &c. The vibrations may be increased by giving a sufficient impulse through the angle of scapement. But they cannot be more than a certain quantity, otherwise the top N of the crank will strike the rim of the wheel. By placing the pins at the very edge of the wheel, the vibrations may easily be increased to a semicircle. By

placing them at the points of long teeth, the crank may get in between them, and the vibrations extended still farther, perhaps to 240°.

This scapement is unquestionably a very good one; and when equally well executed, should excel Graham's both by having but two acting faces to form (and these of hard steel or of stone), and by allowing us to make the circle of rest exceedingly small without diminishing the acting face of the pallet. This will greatly diminish the friction and the influence of oil. But, on the other hand, we apprehend that it is of very difficult execution. The figure of the pallets, in a manner that shall be susceptible of adjustment and removal for repair, and yet sufficiently accurate and steady, seems to us a very delicate job.

Mr. Cumming, in his Elements of Clock and Watch-work, describes (slightly) pallets of the very same construction, making what he conceives to be considerable improvements in the form of the acting faces and the curves of rest. He has also made some watches with this scapement; but they were so difficult, that few workmen can be found fit for the task; and they are exceedingly delicate, and apt to be put out of order. The connection of the pallets with each other, and with the verge, makes the whole such a contorted figure, that it is easily bent and twisted by any jolt or unskilful handling.

There remains another scapement of this kind, having the tooth of the balance-wheel resting on a cylindrical surface on the axis of the verge during the excursions of the balance beyond the angle of scapement, and which differs somewhat in the application of the maintaining power from all those already described.

This is known by the name of the *Duplex scapement*, and is as follows: Plate IX. fig. 6. represents the essential parts greatly magnified. AD is a portion of the balance-wheel, having teeth *f*, *b*, *g*, at the circumference. These teeth are entirely for producing the *rest* of the wheel, while the ba-

lance is making excursions beyond the scapement. This is effected by means of an agate cylinder *o p q*, on the verge. This cylinder has a notch *o*. When the cylinder turns round in the direction *o p q*, the notch easily passes the tooth B which is resting on the cylindric surface; but when it returns in the direction *q p o*, the tooth B gets into the notch, and follows it, pressing on one side of it till the notch comes into the position *o*. The tooth, being then in the position *b*, escapes from the notch, and another tooth drops on the convex surface of the cylinder at B.

The balance-wheel is also furnished with a set of stout flat-sided pins, standing upright on its rim, as represented by *a*, D. There is also fixed on the verge a larger cylinder GFC above the smaller one *o p q*, with its under surface clear of the wheel, and having a pallet C, of ruby or sapphire, firmly indented into it, and projecting so far as just to keep clear of the pins on the wheel. The position of this cylinder, with respect to the smaller one below it, is such that, when the tooth *b* is escaped from the notch, the pallet C has just passed the pin *a*, which was at A while B rested on the small cylinder; but it moved from A to *a*, while B moved to *b*. The wheel being now at liberty, the pin *a* exerts its pressure on the pallet C in the most direct and advantageous manner, and gives it a strong impulsion, following and accelerating it till another tooth stops on the little cylinder. The angle of scapement depends partly on the projection of the pallet, and partly on the diameter of the small cylinder and the advance of the tooth B into the notch. Independent of the action on the small cylinder, the angle of scapement would be the whole arch of the large cylinder between C and *x*. But *a* stops before it is clear of the pallet, and the arch of impulsion is shortened by all the space that is described by the pin while a tooth moves from B to *b*. It stops at *d*.

We are informed by the best artists, that this scapement gives great satisfaction, and equals, if it do not excel,

Graham's cylindrical scapement. It is easier made, and requires very little oil on the small cylinder, and none at all on the pallet. They say that it is the best for pocket watches, and is coming every day more into repute. Theory seems to accord with this character. The resting cylinder may be made very small, and the direct impulse on the pallet gives it a great superiority over all those already described, where the action on the pallet is oblique, and therefore much force is lost by the influence of oil. But we fear that much force is lost by the tooth B shifting its place, and thus shortening the arch of impulsion: for we cannot reckon much on the action of B on the side of the notch, because the lever is so extremely short. Accordingly, all the watches which we have seen of this kind have a very strong main spring in proportion to the size and vibration of the balance. If we lessen this diminution of the angle of impulsion, by lessening the cylinder $o\,p\,q$, and by not allowing B to penetrate far into the notch, the smallest inequality of the teeth, or shake in the pivots of the balance or wheel, will cause irregularity, and even uncertainties in the locking and unlocking the wheel by this cylinder.

A scapement exceedingly like this was applied long ago by Dutertre, a French artist, to a pendulum. The only difference is, that in the pendulum scapement the small cylinder is cut through to the centre, half of it only being left; but the pendulum scapement gives a more effective employment of the maintaining power, because the wheel acts on the pallet during the *whole* of the assisted vibration. In a balance scapement, if we attempt to diminish the inefficient motion of the pin from A to a, by lessening the diameter of the small cylinder, the hold given to the tooth in the notch will be so trifling, that the tooth will be thrown out by the smallest play in the pivot holes, or inequality in the length of the teeth.

With this we conclude our account of scapements, where the action of the maintaining power on the balance is sus-

pended during the excursion beyond the angle of impulsion, by making a tooth rest on the surface of a small concentric cylinder. In such scapements, the balance, during its excursions, is almost free from any connection with the wheels, and its isochronism is disturbed by nothing but the friction on this surface —We come now to scapements of more artful construction, in which the balance is really and completely free during the whole of its excursion, being altogether disengaged from the wheel-work. These are called DETACHED SCAPEMENTS. They are of more recent date. We believe that Mr. Le Roi was the first inventor of them, about the year 1748. In the memoirs of the Academy of Paris for that year, and in the Collection of approved Machines and Inventions, we have descriptions of the contrivance. The balance wheel rests on a detent, while the balance is vibrating in perfect freedom. It has a pallet standing out from the centre, which, in the course of vibration, passes close by the point of a tooth of the wheel. At that instant a pin, connected with this pallet, withdraws the detent from the wheel, and the tooth just now mentioned follows the pallet with rapidity, and gives it a smart push forward. Immediately after, another tooth of the wheel meets the other claw of the detent, and the wheel is again locked. When the balance returns, the pin pushes the detent back into its former place, where it again locks the wheel. Then the balance, resuming its first direction, unlocks the wheel, and receives another impulsion from it. Thus the balance is unconnected with the wheels, except while it gets the impulsion, and at the moments of unlocking the wheels.

This contrivance has been reduced to the greatest possible simplicity by the British artists, and seems scarcely capable of farther improvement. The following is one of the most approved constructions. In Plate IX. fig. 7. *a b c* represents the pallet, which is a cylinder of hard steel or stone, having a notch *a b*. A portion of the balance-wheel is represented by AB. It is placed so near to the cylinder,

that the cylinder is no more than clear of *two adjoining* teeth. DE is a long spring, so fixed to the watch-plate at E, as to press very gently on the stop pin G. A small stud F is fixed to that side of the spring that is next to the wheel. The tooth of the wheel rests on this stud, in such a manner that the tooth *a* is just about to touch the cylinder, and the tooth *f* is just clear of it. Another spring, extremely slender, is attached to the spring DE, on the side next the balance-wheel, and claps close to it, but keeping clear of the stud F, and having its point *o* projecting about $\frac{1}{50}$th of an inch beyond its extremity. When the point *a* is pressed towards the wheel, it yields most readily; but, when pressed in the opposite direction, it carries the spring DE along with it. The cylinder being so placed on the verge that the edge *a* of the notch is close by the tooth *a*, a hole is drilled at *i*, close by the projecting point of the slender spring, and a small pin is driven into this hole. This is the whole apparatus: and this situation of the parts corresponds to the quiescent position of the balance.

Now, let the balance be turned out of this position 80 or 90 degrees, in the direction *a b c*. When it is let go, it returns to this position with an accelerated motion. The pin *i* strikes on the projecting point of the slender spring, and, pressing the strong spring DE outward from the wheel, withdraws the stud F from the tooth; and thus unlocks the wheel. The tooth *a* engages in the notch, and urges round the balance. The pin *i* quits the slender spring before the tooth quits the notch: so that when it is clear of the pallet, the wheel is locked again on the stud F, and another tooth *g* is now in the place of *a*, ready to act in the same manner. When the force of the balance is spent, it stops, and then returns toward its quiescent position with a motion continually accelerated. The pin *i* arives at the point *o* of the slender spring, raises it from the strong spring without disturbing the latter, and almost without *being* disturbed by this trifling obstacle; and it goes on, turning in the direction

a b c, till its force is again spent; it stops, returns, again unlocks the wheel, and gets a new impulsion. And in this manner the vibrations are continued. Thus we see a vibration, almost free, maintained in a manner even more simple than the common crutch scapement. The impulse is given direct without any decomposition by oblique action, and it is continued through the *whole* motion of the wheel. No part of this motion is lost, as in Dupleix's scapement, by the *gradual* approach of the tooth to its active position. Very little force is required for unlocking the wheel, because the spring DFE is made slender at the remote end E, so that it turns round E almost like a lever turning on pivots. A sudden twitch of the watch, in the direction *b a*, might chance to unlock the wheel. But this will only derange one vibration, and even that not considerably, because the teeth are so close to the cylinder that the wheel cannot advance till the notch comes round to the place of scapement. A tooth will continue pressing on the cylinder, and by its friction will change a little the extent and duration of a single vibration. The greatest derangement will happen if the wheel should thus unlock by a jolt, while the notch passes through the arch of scapement in the returning vibration. Even this will not greatly derange it, when the watch is clean and vibrating wide; because, in this position, the balance has its greatest momentum, and the direction of the only jolt that can unlock the wheel tends to increase this momentum relatively. In short, considering it theoretically, it seems an almost perfect scapement; and the performance of many of these watches abundantly confirms that opinion. They are known to keep time for many days together, without varying one second from day to day; and this even under considerable variations of the maintaining power. Other detached scapements may equal this, but we scarcely expect any to exceed it; and its simplicity is so much superior to any that we have seen, that, on this account, we are disposed to give it the preference. We do not mean to say that

it is the best for a pocket watch. Perhaps the scapement of Dupleix or Graham may be preferable, as being susceptible of greater strength, and more able to withstand jolts. Yet it is a fact, that some of the watches made in this form by Arnold and others, have kept time in the wonderful manner above-mentioned while carried about in the pocket.

Mr. Mudge of London invented, about the year 1763, another detached scapement, of a still more ingenious construction. It is a counterpart of Mr. Cumming's scapement for pendulums. The contrivance is to this effect. In Plate IX. fig. 8. *a b c* represents the balance. Its axis is bent into a large crank EFGHIK, sufficiently roomy to admit within it two other axes M and L, with the proper cocks for receiving their pivots. The three axes form one straight line. About these smaller axes are coiled two auxiliary springs, in opposite directions, having their outer extremities fixed in the studs A and B. The balance has its spring also, as usual, and the three springs are so disposed that each of them alone would keep the balance at rest in the same position, which we may suppose to be that represented in the figure. The auxiliary springs A and B are connected with the balance only occasionally, by means of the arms *m* and *n* projecting from their respective axes. These arms are catched on opposite sides by the pins *o*, *p*, in the branches of the crank; so that when the balance turns round, it carries one or other of those arms round with it, and, during this motion, it is affected by the auxiliary spring connected with the arm so carried round by it.

Let us suppose that the balance vibrates 120° on each side of its quiescent position *a b c*, so that the radius E *a* acquires, alternately, the positions E *b* and E *c*. The auxiliary springs are connected with the wheels by a common dead-beat pendulum scapement, so that each can be separately wound up about 30°, and retained in that position. Let us also suppose that the spring A has been wound up 30° in

the direction *a b*, by the wheel-work, and that the point *a* of the rim of the balance, having come from *c*, is passing through *a* with its greatest velocity. When the radius E *a* has passed *a* 30° in its course towards *b*, the pin *o* finds the arm *m* in its way, and carries it along with it till *a* gets to *b*. But, by carrying away the arm *m*, it has unlocked the wheel-work, and the spring B is now wound up 30° in the other direction, but has no connection with the balance during this operation. Thus the balance finishes its semivibration *a b* of 120°, opposed by its own spring the whole way, and by the auxiliary spring A through an angle of 90°. It returns to the position E *a*, aided by A and by the balance spring, through an angle of 120°. In like manner, when E*a* has moved 30° toward the position E *c*, the pin *p* meets with the arm *n*, and carries it along with it through an angle of 90°, opposed by the spring B, and then returns to the position E *a*, assisted by the same spring through an arch of 120°.

Thus it appears that the balance is opposed by each auxiliary spring through an angle of 90°, and assisted through an angle of 120°. This difference of action maintains the vibrations, and the necessary winding up of the auxiliary springs is performed by the wheel-work at a time when they are totally disengaged from the balance. No irregularity of the wheel-work can have any influence on the force of the auxiliary springs, and therefore the balance is completely disengaged from all these irregularities, except in the short moment of unlocking the wheel that winds up the springs.

This is a most ingenious construction, and the nearest approach to a free vibration that has yet been thought of. It deserves particular remark that, during the whole of the returning or accelerated semivibration, the united force of the springs is proportional to the distance from the quiescent position. The same may be said of the retarded excursion beyond the angle of impulse: therefore the only deviation of the forces from the law of cycloidal vibration is during

the motion from the quiescent position to the meeting with the auxiliary spring. Therefore, as the forces, on both sides, beyond this angle, are in their due proportion, and the balance always makes such excursions, there seems nothing to disturb the isochronism, whether the vibrations are wide or narrow. Accordingly, the performance of this scapement, under the severest trials, equalled any that were compared with it, in as far as it depended on scapement alone. But it is evident that the execution of this scapement, though most simple in principle, must always be vastly more difficult than the one described before. There is so little room, that the parts must be exceedingly small, requiring the most accurate workmanship. We think that it may be greatly simplified, preserving all its advantages, and that the parts may be made of more than twice their present size, with even less load on the balance from the inertia of matter. This improvement is now carrying into effect by a friend.

Still, however, we do not see that this scapement is, theoretically, superior to the last. The irregularities of maintaining power affect that scapement only in the arch of impulsion, where the velocity is great, and the time of action very small. Moreover, the chief effect of the irregularities is only to enlarge the excursions; and in these the wheels have no concern.

Mr. Mudge has also given another detached scapement, which he recommends for pocket watches, and executed entirely to his satisfaction in one made for the Queen. A dead beat pendulum scapement is interposed, as in the last, between the wheels and the balance. The crutch EDF (Plate IX. fig. 9.) has a third arm DG, standing outwards from the meeting of the other two, and of twice their length. This arm terminates in a fork AGB. The verge V has a pallet C, which, when all is at rest, would stand between the points A, B of the fork. But the wheel, by its action on the pallet E, forces the fork into the position B *g b*, the point A of the fork being now where B was before, just touching

the cylindrical surface of the verge. The scapement of the crutch EDF is not accurately a dead beat scapement, but has a very small recoil beyond the angle of impulsion. By this circumstance, the branch A (now at B) is made to press most gently on the cylinder, and keeps the wheel locked, while the balance is going round in the direction BHA. The point A gets moving from A to B by means of a notch in the cylinder, which turns round at the same time by the action of the branch AG on the pallet C; but A does not touch the cylinder during this motion, the notch leaving free room for its passage. When the balance returns from its excursion, the pallet C strikes on the branch A (still at B), and unlocks the wheel. This now acting on the crutch pallet F, causes the branch *b* of the fork to follow the pallet C, and give it a strong impulse in the direction in which it is then moving, causing the balance to make a semivibration in the direction AHB. The fork is now in the situation A *g a*, similar to B *g b*, and the wheel is again locked on the crutch pallet E.

The intelligent reader will admit this to be a very steady and effective scapement. The lockage of the wheel is procured in a very ingenious manner; and the friction on the cylinder, necessary for effecting this, may be made as small as we please, notwithstanding a very strong action of the wheel: For the pressure of the fork on the cylinder depends entirely on the degree of recoil that is formed on the pallets E and F. *Pressure* on the cylinder is not *indispensably* necessary, and the crutch scapement might be a real dead beat. But a small recoil, by keeping the fork in contact with the cylinder, gives the most perfect steadiness to the motion. The ingenious inventor, a man of approved integrity and judgment, declares that her Majesty's watch was the best pocket watch he had ever seen. We are not disposed to question its excellency. We saw an experiment watch of this construction, made by a country artist, having a balance so heavy as to vibrate only twice in a second. Every

vibration was sensibly beyond a turn and a half, or 540°. The artist assured us, that when its proper balance was in, vibrating somewhat more than five times in a second, the vibrations even exceeded this. He had procured it this great mobility by substituting a roller with fine pivots in place of the simple pallet of Mudge. This great extent of detached vibration is an unquestionable excellence, and is peculiar to those two scapements of this ingenious artist.

Very ingenious scapements have been made by Ernshaw, Howel, Hayley, and other British artists; and many by the artists of Paris and Geneva. But we must conclude the article, having described all that have any difference in principle.

The scapement having been brought to this degree of perfection, we have an opportunity of making experiments on the law of action of springs, which has been too readily assumed. We think it easy to demonstrate, that the figure of a spring, which must have a great extent of rapid motion, will have a considerable influence on the force which it impresses on a balance *in actual motion*. The accurate determination of this influence is not very difficult in some simple cases. It is the greatest of all in the plane spiral, and the least in the cylindrical; and, in this last form, it is so much less as the diameter is less, the length of the spring being the same. By employing many turns, in order to have the same ultimate force at the extremity of the excursion, this influence is increased. A particular length of spring, therefore, will make it equal to a given quantity; and it may thus compensate for a particular magnitude of friction, and other obstructions. This accounts for the observation of Le Roy, who found that every spring, *when applied to a movement*, had a certain length, which made the wide and narrow vibrations isochronous. His method of trial was so judicious, that there can be no doubt of the justness of his conclusion. His time-keeper had no fuzee; and when the last revolution of the main wheel was going on, the vi-

brations were but of half the extent of those made during the first revolution. Without minding the real rate of going, he only compared the duration of the first and last revolution of the minute hand. An artist of our acquaintance repeated these experiments, and with the same result: But, unfortunately, could derive little benefit from them; because in one state of the oil, or with one balance, he found the lengths of the same spring, which produced isochronous vibrations, were different from those which had this effect in another state of the oil, or with another balance. He also observed another difference in the rate, arising from a difference of position, according as XII, VI, III, or IX, was uppermost; which difference plainly arises from the swagging of the spring by its weight, and, in that state, acting as a pendulum. This unluckily put a stop to his attempts to lessen this hurtful influence by employing a cylindrical spiral of small diameter and great length *.

* It is much to be regretted, that the preceding dissertation is the only one that Dr. Robison has written, upon the very interesting subject of Timekeepers, which he had studied with particular attention. So early as the year 1793 he had projected a magnificent work on the History, Theory, and Practice of Horology; and in a letter addressed to Mr. Thomas Reid of Edinburgh, and containing an account of the plan and object of the work, he requested that this able and experienced artist would co-operate with him in the undertaking. Mr. Reid's occupations did not permit him to agree to this flattering request; and it was probably on this account that Dr. Robison abandoned the work. Many valuable materials respecting the history of Horology, and many profound views respecting its principles, have thus been lost to science. It is fortunate, however, that the practical part which Mr. Reid would have contributed, has been lately published in the article HOROLOGY, which he has written for the EDINBURGH ENCYCLOPÆDIA. ED.

SEAMANSHIP.

419. By this word we express that noble art, or, more purely, the qualifications which enable a man to exercise the noble art of working a ship. A SEAMAN, in the language of the profession, is not merely a mariner or labourer on board a ship, but a man who understands the structure of this wonderful machine, and every subordinate part of its mechanism, so as to enable him to employ it to the best advantage for pushing her forward in a particular direction, and for avoiding the numberless dangers to which she is exposed by the violence of the winds and waves. He also knows what courses can be held by the ship, according to the wind that blows, and what cannot, and which of these is most conducive to her progress in her intended voyage: and he must be able to perform every part of the necessary operation with his own hands. As the seamen express it, he must be able "to hand, reef, and steer."

420. We are justified in calling it a *noble art*, not only by its importance, which it is quite needless to amplify or embellish, but by its immense extent and difficulty, and the prodigious number and variety of principles on which it is

founded—all of which must be possessed in such a manner that they shall offer themselves without reflection in an instant, otherwise the pretended seaman cannot be trusted on his watch.

The art is practised by persons without what we call *education*, and therefore it suffers in the estimation of the careless spectator. It is thought little of, because little attention is paid to it. But if multiplicity, variety, and intricacy of principles, and a systematic knowledge of these principles, entitle any art to the appellation of *scientific* and *liberal*, seamanship claims these epithets in an eminent degree.

421. What a pity it is that an art so important, so difficult, and so intimately connected with the invariable laws of mechanical nature, should be so held by its possessors, that it cannot improve, but must die with each individual. Having no advantages of previous education, they cannot arrange their thoughts; they can hardly be said to think. They can far less express or communicate to others the intuitive knowledge which they possess; and their art, acquired by habit alone, is little different from an instinct. We are as little entitled to expect improvement here as in the architecture of the bee or the beaver. Yet a ship is a machine. We know the forces which act on it, and we know the results of its construction—all these are as fixed as the laws of motion. What hinders this to be reduced to a set of practical maxims, as well founded and as logically deduced as the working of a steam engine or a cotton mill? May not the ingenious speculatist in his closet unravel the intricate thread of mechanism which connects all the manual operations with the unchangeable laws of nature, and both furnish the seaman with a better machine, and direct him to a more dexterous use of it?

422. We cannot help thinking that much may be done; nay, we may say that much has been done. We think highly of the progressive labours of Renaud, Pitot, Bouguer, Du Hamel, Groignard, Bernoulli, Euler, Romme, and others;

and are both surprised and sorry that Britain has contributed so little in these attempts. Gordon is the only one of our countrymen who has given a professedly scientific treatise on a small branch of the subject. The government of France has always been strongly impressed with the notion of great improvements being attainable by systematic study of this art; and we are indebted to the endeavours of that ingenious nation for any thing of practical importance that has been obtained. M. Bouguer was professor of hydrology at one of the marine academies of France, and was enjoined, as part of his duty, to compose dissertations both on the construction and the working of ships. His *Traité du Navire*, and his *Manœuvre des Vaisseaux*, are undoubtedly very valuable performances: So are those of Euler and Bernoulli, considered as mathematical dissertations, and they are wonderful works of genius, considered as the productions of persons who hardly ever saw a ship, and were totally unacquainted with the profession of a seaman. In this respect Bouguer had great superiority, having always lived at a seaport, and having made many very long voyages. His treatises therefore are infinitely better accommodated to the demands of the seaman, and more directly instructive; but still the author is more a mathematician than an artist, and his performance is intelligible only to mathematicians. It is true, the academical education of the young gentlemen of the French navy is such, that a great number of them may acquire the preparatory knowledge that is necessary; and we are well informed that, in this respect, the officers of the British navy are greatly inferior to them.

423. But this very circumstance has furnished to many persons an argument against the utility of those performances. It is said that, "notwithstanding this superior mathematical education, and the possession of those boasted performances of M. Bouguer, the French are greatly inferior, in point of seamanship, to our countrymen, who have not a page in their language to instruct them, and who could not

peruse it if they had it." Nay, so little do the French themselves seem sensible of the advantage of these publications, that no person among them has attempted to make a familiar abridgment of them, written in a way fitted to attract attention; and they still remain neglected in their original abstruse and uninteresting form.

424. We wish that we could give a satisfactory answer to this observation. It is just, and it is important. These very ingenious and learned dissertations are by no means so useful as we should expect. They are large books, and appear to contain much; and as their plan is logical, it seems to occupy the whole subject, and therefore to have done almost all that can be done. But, alas! they have only opened the subject, and the study is yet in its infancy. The whole science of the art must proceed on the knowledge of the impulsions of the wind and water. These are the forces which act on the machine; and its motions, which are the ultimatum of our research, whether as an end to be obtained or as a thing to be prevented, must depend on these forces. Now it is with respect to this fundamental point that we are as yet almost totally in the dark. And, in the performances of M. Bouguer, as also in those of the other authors we have named, the theory of these forces, by which their quantity and the direction of their action are ascertained, is altogether erroneous; and its results deviate so enormously from what is observed in the motions of a ship, that the person who should direct the operations on shipboard, in conformity to the maxims deducible from M. Bouguer's propositions, would be baffled in most of his attempts, and be in danger of losing the ship. The whole proceeds on the supposed truth of that theory which states the impulse of a fluid to be in the proportion of the square of the sine of the angle of incidence; and that its action on any small portion, such as a square foot of the sails or hull, is the same as if that portion were detached from the rest, and were exposed, single and alone, to the wind or water in the same angle. But we

have shown, in the article *Resistance of Fluids*, both from theory and experience, that both of these principles are erroneous, and this to a very great degree, in cases which occur most frequently in practice, that is, in the small angles of inclination. When the wind falls nearly perpendicular on the sails, theory is not very erroneous; but in these cases, the circumstances of the ship's situation are generally such that the practice is easy, occurring almost without thought; and in this case, too, even considerable deviations from the very best practice are of no great moment. The interesting cases, where the intended movement requires or depends upon very oblique actions of the wind on the sails, and its practicability or impracticability depends on a very small variation of this obliquity; a mistake of the force, either as to intensity or direction, produces a mighty effect on the resulting motion. This is the case in sailing to windward; the most important of all the general problems of seamanship. The trim of the sails, and the course of the ship, so as to gain most on the wind, are very nice things; that is, they are confined within very narrow limits, and a small mistake produces a very considerable effect. The same thing obtains in many of the nice problems of tacking, box-hauling, wearing after lying-to in a storm, &c.

The error in the second assertion of the theory is still greater, and the action on one part of the sail or hull is so greatly modified by its action on another adjoining part, that a stay-sail is often seen hanging like a loose rag, although there is nothing between it and the wind; and this merely because a great sail in its neighbourhood sends off a lateral stream of wind, which completely hinders the wind from getting at it. Till the theory of the action of fluids be established, therefore, we cannot tell what are the forces which are acting on every point of the sail and hull: Therefore we cannot tell either the mean intensity or direction of the whole force which acts on any particular sail, nor the intensity and mean direction of the resistance to the hull; cir-

cumstances absolutely necessary for enabling us to say what will be their energy in producing a rotation round any particular axis. In like manner, we cannot, by such a computation, find the spontaneous axis of conversion (see ROTATION), or the velocity of such conversion. In short, we cannot pronounce with tolerable confidence *à priori* what will be the motions in any case, or what dispositions of the sails will produce the movement we wish to perform. The experienced seaman learns by habit the general effects of every disposition of the sails; and though his knowledge is far from being accurate, it seldom leads him into any very blundering operation. Perhaps he seldom makes the best adjustment possible, but seldomer still does he deviate very far from it; and in the most general and important problems, such as working to windward, the result of much experience and many corrections has settled a trim of the sails, which is certainly not far from the truth, but (it must be acknowledged) deviates widely and uniformly from the theories of the mathematician's closet. The honest tar, therefore, must be indulged in his joke on the useless labours of the mathematician, who can neither hand, reef, nor steer.

425. After this account of the theoretical performances in the art of seamanship, and what we have said in another place on the small hopes we entertain of seeing a perfect theory of the impulse of fluids, it will not be expected that we enter very minutely on the subject in this place; nor is it our intention. But let it be observed, that the theory is defective in one point only; and although this is a most important point, and the errors in it destroy the conclusions of the chief propositions, the reasonings remain in full force, and the *modus operandi* is precisely such as is stated in the theory. The *principles* of the art are therefore to be found in these treatises; but false inferences have been drawn, by computing from erroneous quantities. The rules and the practice of the computation, however, are still beyond con-

troversy: Nay, since the process of investigation is legitimate, we may make use of it in order to discover the very circumstance in which we are at present mistaken: for by converting the proposition, instead of finding the motions by means of the supposed forces, combined with the known mechanism, we may discover the forces by means of this mechanism and the observed motions.

426. We shall therefore in this place give a very general view of the movements of a ship under sail, showing how they are produced and modified by the action of the wind on her sails, the water on her rudder and on her bows. We shall not attempt a precise determination of any of these movements: but we shall say enough to enable the curious landsman to understand how this mighty machine is managed amidst the fury of the winds and waves: and, what is more to our wish, we hope to enable the uninstructed but thinking seaman to generalise that knowledge which he possesses: to class his ideas, and give them a sort of rational system: and even to improve his practice, by making him sensible of the immediate operation of every thing he does, and in what manner it contributes to produce the movement which he has in view.

427. A ship may be considered at present as a mass of inert matter in free space, at liberty to move in every direction, according to the forces which impel or resist her: and when she is in actual motion, in the direction of her course, we may still consider her as at rest in absolute space, but exposed to the impulse of a current of water moving equally fast in the opposite direction; for in both cases the pressure of the water on her bows is the same: and we know that it is possible, and frequently happens in currents, that the impulse of the wind on her sails, and that of the water on her bows, balance each other so precisely, that she not only does not stir from the place, but also remains steadily in the same position, with her head directed to the same point of the compass. This state of things is easily conceiv-

ed by any person accustomed to consider mechanical subjects, and every seaman of experience has observed it. It is of importance to consider it in this point of view, because it gives us the most familiar notion of the manner in which these forces of the wind and water are set in opposition, and made to balance or not to balance each other by the intervention of the ship, in the same manner as the goods and the weights balance each other in the scales by the intervention of a beam or steel-yard.

428. When a ship proceeds steadily in her course, without changing her rate of sailing, or varying the direction of her head, we must in the first place conceive the accumulated impulses of the wind on all her sails as precisely equal and directly opposite to the impulse of the water on her bows. In the next place, because the ship does not change the direction of her keel, she resembles the balanced steel-yard, in which the energies of the two weights, which tend to produce rotations in opposite directions, and thus to change the position of the beam, mutually balance each other round the fulcrum; so the energies of the actions of the wind on the different sails balance the energies of the water on the different parts of the hull.

429. The seaman has two principal tasks to perform. The first is to keep the ship steadily in that course which will bring her farthest on in the line of her intended voyage. This is frequently very different from that line, and the choice of the best course is sometimes a matter of considerable difficulty. It is sometimes possible to shape the course precisely along the line of the voyage; and yet the intelligent seaman knows that he will arrive sooner, or with greater safety, at his port, by taking a different course; because he will gain more by increasing his speed than he loses by increasing the distance. Some principle must direct him in the selection of this course. This we must attempt to lay before the reader.

Having chosen such a course as he thinks most advantageous, he must set such a quantity of sail as the strength of the wind will allow him to carry with safety and effect, and must trim the sails properly, or so adjust their positions to the direction of the wind, that they may have the greatest possible tendency to impel the ship in the line of her course, and to keep her steadily in that direction.

His other task is to produce any deviations which he sees proper from the present course of the ship; and to produce these in the most certain, the safest, and the most expeditious manner. It is chiefly in this movement that the mechanical nature of a ship comes into view, and it is here that the superior address and resource of an expert seaman is to be perceived.

It is perfectly consonant to experience that the impulse of fluids is in the duplicate ratio of the relative velocity. Let it be supposed that when water moves one foot per second its perpendicular pressure or impulse on a square foot is m pounds. Then, if it be moving with the velocity V estimated in feet per second, its perpendicular impulse on a surface S, containing any number of square feet, must be $m\,SV^2$.

In like manner, the impulse of air on the same surface may be represented by $n\,SV^2$: and the proportion of the impulse of these two fluids will be that of m to n. We may express this by the ratio of q to 1 making $\frac{m}{n} = q$.

430. M. Bouguer's computations and tables are on the supposition that the impulse of sea-water moving one foot per second is 23 ounces on a square foot, and that the impulse of the wind is the same when it blows at the rate of 24 feet per second. These measures are all French. They by no means agree with the experiments of others; and what we have already said, when treating of the RESISTANCE of Fluids, is enough to shew us that nothing like precise measures can be expected. It was shown as the result of a ra-

tional investigation, and confirmed by the experiments of Buat and others, that the impulsions and resistances at the same surface, with the same obliquity of incidence and the same velocity of motion, are different according to the form and situation of the adjoining parts. Thus the total resistance of a thin board is greater than that of a long prism, having this board for its front or bow, &c.

We are greatly at a loss what to give as absolute measures of these impulsions.

1. With respect to water. The experiments of the French academy on a prism two feet broad and deep, and four feet long, indicate a resistance of 0,973 pounds avoirdupois to a square foot, moving with the velocity of one foot per second at the surface of still water.

Mr. Buat's experiments on a square foot wholly immersed in a stream were as follow:

A square foot as a thin plate -	1,81 pounds.
Ditto as the front of a box one foot long	1,42
Ditto as the front of a box three feet long	1,29

The resistance of sea-water is about $\frac{1}{23}$ greater.

2. With respect to air, the varieties are as great. The resistance of a square foot to air moving with the velocity of one foot per second appears from Mr. Robins's experiments on 16 square inches to be on a square foot 0,001596 pounds.

Chevalier Borda's on 16 inches	0,001757
——————— on 81 inches	0,002042
Mr. Rouse's on large surfaces	0,002291

Precise measures are not to be expected, nor are they necessary in this inquiry. Here we are chiefly interested in their proportions, as they may be varied by their mode of action in the different circumstances of obliquity and velocity.

431. We begin by recurring to the fundamental proposition concerning the impulse of fluids, viz. that the absolute pressure is always in a direction perpendicular to the impel-

led surface, whatever may be the direction of the stream of fluid. We must therefore illustrate the doctrine, by always supposing a flat surface of sail stretched on a yard, which can be braced about in any direction, and giving this sail such a position and such an extent of surface that the impulse on it may be the same both as to direction and intensity with that on the real sails. Thus the consideration is greatly simplified. The direction of the impulse is therefore perpendicular to the yard. Its intensity depends on the velocity with which the wind meets the sail, and the obliquity of its stroke. We shall adopt the constructions founded on the common doctrine, that the impulse is as the square of the sine of the inclination, because they are simple; whereas, if we were to introduce the values of the oblique impulses, such as they have been observed in the excellent experiments of the Academy of Paris, the constructions would be complicated in the extreme, and we could hardly draw any consequences which would be intelligible to any but expert mathematicians. The conclusions will be erroneous not in kind but in quantity only: and we shall point out the necessary corrections, so that the final results will be found not very different from real observation.

432. If a ship were a round cylindrical body like a flat tub, floating on its bottom, and fitted with a mast and sail in the centre, she would always sail in a direction perpendicular to the yard. This is evident. But she is an oblong body, and may be compared to a chest, whose length greatly exceeds its breadth. She is so shaped, that a moderate force will push her through the water with the head or stern foremost; but it requires a very great force to push her sidewise with the same velocity. A fine sailing ship of war will require about 12 times as much force to push her sidewise as to push her head foremost. In this respect therefore she will very much resemble a chest whose length is 12 times its breadth; and whatever be the proportion of these resistances in different ships, we may always substitute

box which shall have the same resistances headwise and sidewise.

433. Let EFGH (Plate X. fig. 1.) be the horizontal section of such a box, and AB its middle line, and C its centre. In whatever direction this box may chance to move, the direction of the whole resistance on its two sides will pass through C. For as the whole stream has one inclination to the side EF, the equivalent of the equal impulses on every part will be in a line perpendicular to the middle of EF. For the same reason, it will be in a line perpendicular to the middle of FG. These perpendiculars must cross in C Suppose a mast erected at C, and YC*y* to be a yard hoisted on it carrying a sail. Let the yard be first conceived as braced right athwart at right angles to the keel, as represented by Y′ *y*′. Then, whatever be the direction of the wind abaft this sail, it will impel the vessel in the direction CB. But if the sail has the oblique position Y *y*, the impulse will be in the direction CD perpendicular to CY, and will both push the vessel ahead and sidewise: For the impulse CD is equivalent to the two impulses CK and CI (the sides of a rectangle of which CD is the diagonal). The force CI pushes the vessel ahead, and CK pushes her sidewise. She must therefore take some intermediate direction *a b*, such that the resistance of the water to the plane FG is to its resistance to the plane EF as CI to CK.

The angle *b* CB between the real course and the direction of the head is called the LEEWAY; and in the course of this dissertation we shall express it by the symbol *x*. It evidently depends on the shape of the vessel and on the position of the yard. An accurate knowledge of the quantity of leeway, corresponding to different circumstances of obliquity of impulse, extent of surface, &c. is of the utmost importance in the practice of navigation; and even an approximation is valuable. The subject is so very difficult that this must content us for the present.

434. Let V be the velocity of the ship in the direction C b, and let the surfaces FG and FE be called A′ and B Then the resistance to the lateral motion is $m\,V^2 \times B' \times$ sine^2, b CB, and that to the direct motion is $m\,V^2 \times A' \times$ sine^2, b CK, or $m\,V^2 \times A' \times$ cos.^{2} b CB. Therefore thes resistances are in the proportion of $B' \times$ sine^2, x to $A' \times$ cos.^{2}, x (representing the angle of leeway b CB by the sym bol x.)

Therefore we have CI : CK, or CI : ID $= A' \cdot$ cos.^{2} $: B' \cdot \text{sine}^2 x, = A' : B' \cdot \dfrac{\text{sine}^2 x}{\text{cos.}^2 x} = A : B \cdot \text{tangent}^2 x.$

Let the angle YCB, to which the yard is braced up, b called the TRIM of the sails, and expressed by the symbo b. This is the complement of the angle DCI. Now CI ID $=$ rad. : tan. DCI, $= 1 :$ tan. DCI, $= 1 :$ cotan. b. There fore we have finally $1 :$ cotan. $b = A' : B' \cdot$ tan.^{2} x, and A cotan. $b = B' \cdot$ tangent^2 x, and tan.^{2} $x = \dfrac{A}{B}$ cot. b. This equa tion evidently ascertains the mutual relation between th trim of the sails and the leeway in every case where we ca tell the proportion between the resistances to the direct an broadside motions of the ship, and where this proportio does not change by the obliquity of the course. Thu suppose the yard braced up to an angle of 30° with the kee Then cotan. $30° = 1{,}732$ very nearly. Suppose also th the resistance sidewise is 12 times greater than the resis ance headwise. This gives $A' = 1$ and $B' = 12$. Ther fore $1{,}732 = 12 \times$ tangent^2 x, and tangent^2 $x = \dfrac{1{,}732}{12}$, = 0,14434, and tan. $x = 0{,}3799$, and $x = 20° 48'$, very near two points of leeway.

This computation, or rather the equation which giv room for it, supposes the resistances proportional to th squares of the sines of incidence. The experiments of th Academy of Paris, of which an abstract is given in the a ticle *RESISTANCE of Fluids*, show that this supposition is n

far from the truth when the angle of incidence is great. In this present case the angle of incidence on the front FG is about 70°, and the experiments just now mentioned show that the real resistances exceed the theoretical ones only $\frac{1}{180}$. But the angle of incidence on EF is only 20° 48′. Experiment shows that in this inclination the resistance is almost quadruple of the theoretical resistances. Therefore the lateral resistance is assumed much too small in the present instance. Therefore a much smaller leeway will suffice for producing a lateral resistance which will balance the lateral impulse CK, arising from the obliquity of the sail, viz. 30°. The matter of fact is, that a pretty good sailing ship, with her sails braced to this angle at a medium, will not make above five or six degrees leeway in smooth water and easy weather; and yet in this situation the hull and rigging present a very great surface to the wind, in the most improper positions, so as to have a very great effect in increasing her leeway. And if we compute the resistances for this leeway of six degrees by the actual experiments of the French Academy on that angle, we shall find the result not far from the truth; that is, the direct and lateral resistances will be nearly in the proportion of CI to ID.

It results from this view of the matter, that the leeway is in general much smaller than what the usual theory assigns.

435. We also see, that according to whatever law the resistances change by a change of inclination, the leeway remains the same while the trim of the sails is the same. The leeway depends only on the direction of the impulse of the wind; and this depends solely on the position of the sails with respect to the keel, whatever may be the direction of the wind. This is a very important observation, and will be frequently referred to in the progress of the present investigation. Note, however, that we are here considering only the action on the sails, and on the same sails. We are not considering the action of the wind on the hull and rig-

ging. This may be very considerable; and it is always in a lee direction, and augments the leeway; and its influence must be so much the more sensible as it bears a greater proportion to the impulse on the sails. A ship under courses, or close-reefed topsails and courses, must make more leeway than when under all her canvas trimmed to the same angle. But to introduce this additional cause of deviation here, would render the investigation too complicated to be of any use.

436. This doctrine will be considerably illustrated by attending to the manner in which a lighter is tracked along a canal, or swings to its anchor in a stream. The track rope is made fast to some staple or bolt E on the deck (Plate X. fig. 2.), and is passed between two of the timber-heads of the bow at D, and laid hold of at F on shore. The men or cattle walk along the path FG, the rope keeps extended in the direction DF, and the lighter arranges itself in an oblique position AB, and is thus dragged along in the direction *a b*, parallel to the side of the canal. Or, if the canal has a current in the opposite direction *b a*, the lighter may be kept steady in its place by the rope DF made fast to a post at F. In this case, it is always observed that the lighter swings in a position AB, which is oblique to the stream *a b*. Now the force which retains it in this position, and which precisely balances the action of the stream, is certainly exerted in the direction DF; and the lighter would be held in the same manner if the rope were made fast at C amidship, without any dependence on the timberheads at D; and it would still be held in the same position, if, instead of the single rope CF, it were riding by two ropes CG and CH, of which CH is in a direction right ahead, but oblique to the stream, and the other CG is perpendicular to CH or AB. And, drawing DI and DK perpendicular to AB and CG, the strain on the rope CH is to that on the rope CG as CI to CK. The action of the rope in these cases is precisely analogous to that of the sail *y* Y; and the obliquity of

the keel to the direction of the motion, or to the direction of the stream, is analogous to the leeway. All this must be evident to any person accustomed to mechanical disquisitions.

437. A most important use may be made of this illustration. If an accurate model be made of a ship, and if it be placed in a stream of water, and ridden in this manner by a rope made fast at any point D of the bow, it will arrange itself in some determined position AB. There will be a certain obliquity to the stream, measured by the angle B *o b;* and there will be a corresponding obliquity of the rope, measured by the angle FCB. Let *y* CY be perpendicular to CF. Then CY will be the position of the yard, or trim of the sails corresponding to the leeway *b* CB. Then, if we shift the rope to a point of the bow distant from D by a small quantity, we shall obtain a new position of the ship, both with respect to the stream and the rope; and in this way may be obtained the relation between the position of the sails and the leeway, independent of all theory, and susceptible of great accuracy; and this may be done with a variety of models suited to the most usual forms of ships.

438. In farther thinking on this subject, we are persuaded that these experiments, instead of being made on models, may with equal ease be made on a ship of any size. Let the ship ride in a stream at a mooring D (Plate X. fig. 3.) by means of a short hawser BCD from her bow, having a spring AC on it carried out from her quarter. She will swing to her moorings, till she ranges herself in a certain position AB with respect to the direction *a b* of the stream; and the hawser DC will be directed to some point E in the line of the keel. Now, it is plain to any person acquainted with mechanical disquisitions, that the deviation BE *b* is precisely the leeway that the ship will make when the average position of the sails is that of the line GEH perpendicular to ED; at least this will give the leeway which is produced by the sails alone. By heaving on the spring,

the knot C may be brought into any other position we please; and for every new position of the knot the ship will take a new position with respect to the stream and to the hawser. And we persist in saying, that more information will be got by this train of experiments than from any mathematical theory: for all theories of the impulses of fluids must proceed on physical postulates with respect to the motions of the filaments, which are exceedingly conjectural.

439. And it must now be farther observed, that the substitution which we have made of an oblong parallelopiped for a ship, although well suited to give us clear notions of the subject, is of small use in practice: for it is next to impossible (even granting the theory of oblique impulsions) to make this substitution. A ship is of a form which is not reducible to equations; and therefore the action of the water on her bow or broadside can only be had by a most laborious and intricate calculation for almost every square foot of its surface. (See *Bezout's Cours de Mathem.* vol. 5. p. 72, &c.) And this must be different for every ship. But, which is more unlucky, when we have got a parallelopiped which will have the same proportion of direct and lateral resistance for a particular angle of leeway, it will not answer for another leeway of the same ship; for when the leeway changes, the figure actually exposed to the action of the water changes also. When the leeway is increased, more of the lee-quarter is acted on by the water, and a part of the weather-bow is now removed from its action. Another parallelopiped must therefore be discovered, whose resistances shall suit this new position of the keel with respect to the real course of the ship.

We therefore beg leave to recommend this train of experiments to the notice of the ASSOCIATION FOR THE IMPROVEMENT OF NAVAL ARCHITECTURE as a very promising method for ascertaining this important point. And we proceed, in the next place, to ascertain the relation between the velocity of the ship and that of the wind, modified as

they may be by the trim of the sails and the obliquity of the impulse.

440. Let AB (Plate X. fig. 4, 5, and 6.) represent the horizontal section of a ship. In place of all the drawing sails, that is, the sails which are really filled, we can always substitute one sail of equal extent, trimmed to the same angle with the keel. This being supposed attached to the yard DCD, let this yard be first of all at right angles to the keel, as represented in Plate X. fig. 4. Let the wind blow in the direction WC, and let CE (in the direction WC continued) represent the velocity V of the wind. Let CF be the velocity v of the ship. It must also be in the direction of the ship's motion, because when the sail is at right angles to the keel, the absolute impulse on the sail is in the direction of the keel, and there is no lateral impulse, and consequently no leeway. Draw EF, and complete the parallelogram CFE e, producing e C through the centre of the yard to w. Then w C will be the relative or apparent direction of the wind, and C e or FE will be its apparent or relative velocity: For if the line C e be carried along CF, keeping always parallel to its first position, and if a particle of air move uniformly along CE (a fixed line in absolute space) in the same time, this particle will always be found in that point of CE where it is intersected at that instant by the moving line C e; so that if C e were a tube, the particle of air, which really moves in the line CE, would always be found in the tube C e. While CE is the real direction of the wind, C e will be the position of the vane at the mast head, which will therefore mark the apparent direction of the wind, or its motion relative to the moving ship.

We may conceive this in another way. Suppose a cannon shot fired in the direction CE at the passing ship, and that it passes through the mast at C with the velocity of the wind. It will not pass through the offside of the ship at P, in the line CE: for while the shot moves from C to P, the point P has gone forward, and the point p is now in the

place where P was when the shot passed through the mast. The shot will therefore pass through the ship's side in the point *p*, and a person on board seeing it pass through C and *p* will say that its motion was in the line C *p*.

441. Thus it happens, that when a ship is in motion the apparent direction of the wind is always ahead of its real direction. The line *w* C is always found within the angle WCB. It is easy to see from the construction, that the difference between the real and apparent directions of the wind is so much more remarkable as the velocity of the ship is greater: For the angle WC *w* or EC *e* depends on the magnitude of E *e* or CF, in proportion to CE. Persons not much accustomed to attend to these matters are apt to think all attention to this difference to be nothing but affectation of nicety. They have no notion that the velocity of a ship can have any sensible proportion to that of the wind. "Swift as the wind" is a proverbial expression; yet the velocity of a ship always bears a very sensible proportion to that of the wind, and even very frequently exceeds it. We may form a pretty exact notion of the velocity of the wind by observing the shadows of the summer clouds flying along the face of a country, and it may be very well measured by this method. The motion of such clouds cannot be very different from that of the air below; and when the pressure of the wind on a flat surface, while blowing with a velocity measured in this way, is compared with its pressure when its velocity is measured by more unexceptionable methods, they are found to agree with all desirable accuracy. Now observations of this kind frequently repeated, show that what we call a pleasant brisk gale blows at the rate of about 10 miles an hour, or about 15 feet in a second, and exerts a pressure of half a pound on a square foot. Mr. Smeaton has frequently observed the sails of a windmill, driven by such a wind, moving faster, nay much faster, towards their extremities, so that the sail, instead of being pressed to the frames on the arms, was taken aback, and fluttering on them. Nay,

we know that a good ship, with all her sails set and the wind on the beam, will in such a situation sail above 10 knots an hour in smooth water. There is an observation made by every experienced seaman, which shows this difference between the real and apparent directions of the wind very distinctly. When a ship that is sailing briskly with the wind on the beam tacks about, and then sails equally well on the other tack, the wind always appears to have shifted and come more ahead. This is familiar to all seamen. The seaman judges of the direction of the wind by the position of the ship's vanes. Suppose the ship sailing due west on the starboard tack, with the wind apparently N. N. W. the vane pointing S. S. E. If the ship puts about, and stands due east on the larboard tack, the vane will be found no longer to point S. S. E. but perhaps S. S. W. the wind appearing N. N. E. and the ship must be nearly closehauled in order to make an east course. The wind appears to have shifted four points. If the ship tacks again, the wind returns to its old quarter. We have often observed a greater difference than this. The celebrated astronomer Dr. Bradley, taking the amusement of sailing in a pinnace on the river Thames, observed this, and was surprised at it, imagining that the change of wind was owing to the approaching to or retiring from the shore. The boatmen told him that it always happened at sea, and explained it to him in the best manner they were able. The explanation struck him, and set him a musing on an astronomical phenomenon which he had been puzzled by for some years, and which he called THE ABERRATION OF THE FIXED STARS. Every star changes its place a small matter for half a year, and returns to it at the completion of the year. He compared the stream of light from the star to the wind, and the telescope of the astronomer to the ship's vane, while the earth was like the ship, moving in opposite directions when in the opposite points of its orbit. The telescope must always be pointed ahead of the real direction of the star, in the same

manner as the vane is always in a direction ahead of the wind; and thus he ascertained the progressive motion of light, and discovered the proportion of its velocity to the velocity of the earth in its orbit, by observing the deviation which was necessarily given to the telescope. Observing that the light shifted its direction about 40″, he concluded its velocity to be about 11,000 times greater than that of the earth; just as the intelligent seaman would conclude from this apparent shifting of the wind, that the velocity of the wind is about triple that of the ship. This is indeed the best method for discovering the velocity of the wind. Let the direction of the vane at the mast-head be very accurately noticed on both tacks, and let the velocity of the ship be also accurately measured. The angle between the directions of the ship's head on these different tacks being halved, will give the real direction of the wind, which must be compared with the position of the vane in order to determine the angle contained between the real and apparent directions of the wind or the angle EC *e*; or half of the observed shifting of the wind will show the inclination of its true and apparent directions. This being found, the proportion of EC to FC (Plate X. fig. 6.) is easily measured.

We have been very particular on this point, because since the mutual actions of bodies depend on their relative motions only, we should make prodigious mistakes if we estimated the action of the wind by its real direction and velocity, when they differ so much from the relative or apparent.

442. We now resume the investigation of the velocity of the ship (Plate X. fig. 4.), having its sail at right angles to the keel, and the wind blowing in the direction and with the velocity CE, while the ship proceeds in the direction of the keel with the velocity CF. Produce E *e*, which is parallel to BC, till it meet the yard in *g*, and draw FG perpendicular to E *g*. Let *a* represent the angle WCD, contained between the sail and the real direction of the wind, and let *b*

be the angle of trim DCB. CE the velocity of the wind was expressed by V, and CF the velocity of the ship by v.

The absolute impulse on the sail is (by the usual theory) proportional to the square of the relative velocity, and to the square of the sine of the angle of incidence; that is, to $FE^2 \times \sin.^2 w$ CD. Now the angle $GFE = w$ CD, and EG is equal to FE $\times$ sin. GFE; and EG is equal to $Eg - g$ G. But $Eg = EC \times \sin. EC\,g$, $= V \times \sin. a$; and $g\,G = CF$, $= v$. Therefore $EG = V \times \sin. a - v$, and the impulse is proportional to $\overline{V \times \sin. a - v}^2$. If S represent the surface of the sail, the impulse, in pounds, will be n S $(V \times \sin. a - v)^2$.

Let A be the surface which, when it meets the water perpendicularly with the velocity v, will sustain the same pressure or resistance which the bows of the ship actually meets with. This impulse, in pounds, will be m A v^2. Therefore, because we are considering the ship's motion as in a state of uniformity, the two pressures balance each other; and therefore $mAv^2 = n$ S $(V \times \sin. a - v)^2$, and $\frac{m}{n} Av^2 = S(V \times \sin. a - v)^2$; therefore $\sqrt{\frac{m}{n}} \sqrt{A} \times v = \sqrt{S} \times V \times \sin. a - v \sqrt{S}$, and

$$v = \frac{\sqrt{S} \times V \times \sin. a}{\sqrt{\frac{m}{n}A} + \sqrt{S}} = \frac{V \times \sin. a}{\sqrt{\frac{m\,A}{n\,S}} + 1} = \frac{V \times \sin. a}{\sqrt{\frac{qA}{S}} + 1}.$$

We see, in the first place, that the velocity of the ship is *(cæteris paribus)* proportional to the velocity of the wind, and to the sine of its incidence on the sail jointly; for while the surface of the sail S and the equivalent surface for the bows remains the same, v increases or diminishes at the same rate with V· sin. a. When the wind is right astern, the sine of a is unity, and then the ship's velocity is

$$\frac{V}{\sqrt{\frac{m\,A}{n\,S}} + 1}.$$

Now, that the denominator of this fraction is a common number; for m and n are numbers, and A and S being quantities of one kind, $\frac{A}{S}$ is also a number.

It must also be carefully attended to, that S expresses a quantity of sail actually receiving wind with the inclination a. It will not always be true, therefore, that the velocity will increase as the wind is more abaft, because some sails will then become others. This observation is not, however, of great importance; for it is very unusual to put a ship in the situation considered hitherto; that is, with the yards square, unless she be right before the wind.

If we would discover the relation between the velocity and the quantity of sail in this simple case of the wind right aft, observe that the equation $v = \frac{V}{\sqrt{\frac{m}{n}\frac{A}{S}+1}}$ gives us $\sqrt{\frac{m}{n}\frac{A}{S}}\,v + v = V$, and $\sqrt{\frac{m}{n}\frac{A}{S}}\,v = V - v$, and $\frac{m}{n}\frac{A}{S}\,v^2 = \overline{V-v}^2$, and $\frac{n}{m}\frac{S}{A} = \frac{v^2}{(V-v)^2}$; and because n and m and A are constant quantities, S is proportional to $\frac{v^2}{(V-v)^2}$; or the surface of sail is proportional to the square of the ship's velocity directly, and to the square of the relative velocity inversely. Thus, if a ship be sailing with $\frac{1}{2}$ of the velocity of the wind, and we would have her sail with $\frac{1}{4}$ of it, we must quadruple the sails. This is more easily seen in another way. The velocity of the ship is proportional to the velocity of the wind, and therefore the relative velocity is also proportional to that of the wind, and the impulse of the wind is as the square of the relative velocity. Therefore, in order to increase the relative velocity by an increase of sail only, we must make this increase of sail in the duplicate proportion of the increase of velocity.

Let us, in the next place, consider the motion of a ship whose sails stand oblique to the keel.

443. The construction for this purpose differs a little from the former, because, when the sails are trimmed to any oblique position DCB (Plate X. fig. 5. and 6.), there must be a deviation from the direction of the keel, or a leeway BCb. Call this x. Let CF be the velocity of the ship. Draw, as before, Eg perpendicular to the yard, and FG perpendicular to Eg; also draw FH perpendicular to the yard: then, as before, EG, which is in the subduplicate ratio of the impulse on the sail, is equal to Eg — Gg. Now Eg is, as before, $= V \times \sin.\ a$, and Gg is equal to FH, which is $=$ CF $\times$ sin. FCH, or $= v \times \sin.\ (b + x)$. Therefore we have the impulse $= n\,S\,(V \cdot \sin.\ a - v \cdot \sin.\ (b + x))^2$.

This expression of the impulse is perfectly similar to that in the former case, its only difference consisting in the subductive part, which is here $v \times \sin\ \overline{b + x}$ instead of v. But it expresses the same thing as before, viz. the diminution of the impulse. The impulse being reckoned solely in the direction perpendicular to the sail, it is diminished solely by the sail withdrawing itself *in that direction* from the wind; and as gE may be considered as the real impulsive motion of the wind, GE must be considered as the relative and effective impulsive motion. The impulse would have been the same had the ship been at rest, and had the wind met it perpendicularly with the velocity GE.

444. We must now show the connection between this impulse and the motion of the ship. The sail, and consequently the ship, is pressed by the wind in the direction CI perpendicular to the sail or yard with the force which we have just now determined. This (in the state of uniform motion) must be equal and opposite to the action of the water. Draw IL at right angles to the keel. The impulse in the direction CI (which we may measure by CI) is equivalent to the impulses CL and LI. By the first the ship is impelled right forward, and by the second she is driven sidewise. There-

fore we must have a leeway, and a lateral as well as a direct resistance. We suppose the form of the ship to be known, and therefore the proportion is known, or discoverable, between the direct and lateral resistances corresponding to every angle x of leeway. Let A be the surface whose perpendicular resistance is equal to the direct resistance of the ship corresponding to the leeway x, that is, whose resistance is equal to the resistance really felt by the ship's bows in the direction of the keel when she is sailing with this leeway; and let B in like manner be the surface whose perpendicular resistance is equal to the actual resistance to the ship's motion in the direction LI, perpendicular to the keel. (*N. B.* This is not equivalent to A′ and B′ adapted to the rectangular box, but to A′ $\cdot \cos.^2 x$ and B′ $\cdot \sin.^2 x$.) We have therefore $A \cdot B = CL : LI$, and $LI = \frac{CL \cdot B}{A}$. Also, because $CI = \sqrt{CL^2 + LI^2}$, we have $A : \sqrt{A^2 + B^2} = CL : CI$, and $CI = \frac{CL \cdot \sqrt{A^2 + B^2}}{A}$. The resistance in the direction LC is properly measured by $m\,A\,v^2$, as has been already observed. Therefore the resistance in the direction IC must be expressed by $m \sqrt{A^2 + B^2}\, v^2$; or (making C the surface which is equal to $\sqrt{A^2 + B^2}$, and which will therefore have the same perpendicular resistance to the water having the velocity v) it may be expressed by $m\,C\,v^2$.

Therefore, because there is an equilibrium between the impulse and resistance, we have $m\,C\,v^2 = n\,S\,(V \sin. a - v \sin. \overline{b + x})^2$ and $\frac{m}{n}\,C\,v^2$, or $q\,C\,v^2 = S\,(V \cdot \sin. a - v \cdot \sin. \overline{b + x})^2$, and $\sqrt{q}\,\sqrt{C}\,v = \sqrt{S}\,(V \cdot \sin. a - v. \sin. \overline{b + x}.)$

Therefore $v = \frac{\sqrt{S} \cdot V \cdot \sin. a}{\sqrt{q}\,\sqrt{C} + \sqrt{S} \cdot \sin. \overline{b + x}}$, $= \frac{V \cdot \sin. a}{\sqrt{q}\,\frac{\sqrt{C}}{\sqrt{S}} + \sin. \overline{b + x}}$, $= V \frac{\sin. a}{\sqrt{q}\,\frac{\sqrt{C}}{\sqrt{S}} + \sin. \overline{b + x}}$

Observe that the quantity which is the coefficient of V in this equation is a common number; for sin. a is a number, being a decimal fraction of the radius 1. Sin. $b + x$ is also a number, for the same reason. And since m and n were numbers of pounds, $\frac{m}{n}$ or q is a common number. And because C and S are surfaces or quantities of one kind, $\frac{C}{S}$ is also a common number.

This is the simplest expression that we can think of for the velocity acquired by the ship, though it must be acknowledged to be too complex to be of very prompt use. Its complication arises from the necessity of introducing the leeway x. This affects the whole of the denominator; for the surface C depends on it, because C is $= \sqrt{A^2 + B^2}$, and A and B are analogous to $A' \cos.^2 x$ and $B' \sin.^2 x$.

445. But we can deduce some important consequences from this theorem.

While the surface S of the sail actually filled by the wind remains the same, and the angle DCB, which in future we shall call the TRIM of the sails, also remains the same, both the leeway x and the substituted surface C remain the same. The denominator is therefore constant; and the velocity of the ship is proportional to $\sqrt{S} \cdot V \cdot \sin. a$; that is, directly as the velocity of the wind, directly as the sine of the absolute inclination of the wind to the yard, and directly as the square root of the surface of the sails.

We also learn from the construction of the figure that FG parallel to the yard cuts CE in a given ratio. For CF is in a constant ratio to E g, as has been just now demonstrated. And the angle DCF is constant, Therefore CF $\cdot$ sin. b, or FH or G g, is proportional to E g, and OC to EC, or EC is cut in one proportion, whatever may be the angle ECD, so long as the angle DCF is constant.

We also see that it is very possible for the velocity of the ship on an oblique course to exceed that of the wind. This

will be the case when the number $\frac{\sin. a}{\sqrt{q\frac{C}{S} + \sin. b + x}}$ ex

ceeds unity, or when sin. a is greater than $\sqrt{q\frac{C}{S}} + \sin$

$b + x$. Now this may easily be by sufficiently enlarging S and diminishing $b + x$. It is indeed frequently seen in fine sailers with all their sails set and not hauled too near the wind.

We remarked above that the angle of leeway x affects the whole denominator of the fraction which expresses the velocity. Let it be observed that the angle ICL is the complement of LCD, or of b. Therefore CL : LI, or A : B = 1 : tan. ICL. = 1 : cot. b, and B = A. cotan. b. Now A is equivalent to A' · cos.^{2} x, and thus b becomes a function of x. C is evidently so, being $= \sqrt{A^2 + B^2}$. Therefore before the value of this fraction can be obtained, we must be able to compute, by our knowledge of the form of the ship, the value of A for every angle x of leeway. This can be done only by resolving her bows into a great number of elementary planes, and computing the impulses on each and adding them into one sum. The computation is of immense labour, as may be seen by one example given by Bouguer. When the leeway is but small, not exceeding ten degrees, the substitution of the rectangular prism of one determined form is abundantly exact for all leeways contained within this limit; and we shall soon see reason for being contented with this approximation. We may now make use of the formula expressing the velocity for solving the chief problems in this part of the seaman's task.

446. And first let it be required to determine the best position of the sail for standing on a given course a b, when C E the direction and velocity of the wind, and its angle with the course W C F, are given. This problem has exercised the talents of the mathematicians ever since the day of Newton. In the article PNEUMATICS we gave the solu

tion of one very nearly related to it, namely, to determine the position of the sail which would produce the greatest impulse in the direction of the course. The solution was to place the yard CD in such a position that the tangent of the angle FCD may be one-half of the tangent of the angle DCW. This will indeed be the best position of the sail for beginning the motion; but as soon as the ship begins to move in the direction CF, the effective impulse of the wind is diminished, and also its inclination to the sail. The angle DC w diminishes continually as the ship accelerates; for CF is now accompanied by its equal eE, and by an angle EC e, or WC w. CF increases, and the impulse on the sail diminishes, till an equilibrium obtains between the resistance of the water and the impulse of the wind. The impulse is now measured by C e^2 × sin.^{2} eCD instead of CE^2 × sin.^{2} ECD, that is, by EG^2 instead of E g^2.

This introduction of the relative motion of the wind renders the actual solution of the problem extremely difficult. It is very easily expressed geometrically: Divide the angle wCF in such a manner that the tangent of DCF may be half of the tangent of DC w, and the problem may be constructed geometrically as follows.

Let WCF (Plate X. fig. 7.) be the angle between the sail and course. Round the centre C describe the circle WDFY; produce WC to Q, so that C Q = $\frac{1}{3}$ W C, and draw QY parallel to CF cutting the circle in Y; bisect the arch WY in D, and draw DC. DC is the proper position of the yard.

Draw the chord WY, cutting CD in V, and CF in T: draw the tangent PD cutting CF in S, and CY in R.

It is evident that WY, PR, are both perpendicular to CD, and are bisected in V and D; therefore (by reason of the parallels QY, CF) 4 : 3 = QW : CW, = YW : TW, = RP : SP. Therefore PD : PS = 2 : 3, and PD : DS = 2 : 1. *Q. E. D.* But this division cannot be made to the best advantage till the ship has attained its greatest velocity, and the angle wCF has been produced.

We must consider all the three angles, a, b, and x, as variable in the equation which expresses the value of v, and we must make the fluxion of this equation $= o$; then, by means of the equation $B = A \cdot$ cotan. b, we must obtain the value of b and of $\dot{b}$ in terms of x and $\dot{x}$. With respect to a, observe, that if we make the angle $WCF = p$, we have $p = a + b + x$; and p being a constant quantity, we have $\dot{a} + \dot{b} + \dot{x} = o$. Substituting for a, b, $\dot{a}$, and $\dot{b}$, their values in terms of x and $\dot{x}$, in the fluxionary equation $= o$, we readily obtain x, and then a and b, which solves the problem.

Let it be required, in the next place, to determine the course and the trim of the sails most proper for plying to windward.

447. In Plate X. fig. 6. draw FP perpendicular to WC. CF is the motion of the ship; but it is only by the motion CP that she gains to windward. Now CP is $= CF \times$ cosin. WCF, or $v \cdot$ cosin. $(a + b + x)$. This must be rendered a maximum, as follows.

By means of the equation which expresses the value of v and the equation $B = A \cdot$ cotan. b, we exterminate the quantities v and b; we then take the fluxion of the quantity into which the expression $v \cdot$ cos. $(a + b + x)$ is changed by this operation. Making this fluxion $= o$, we get the equation which must solve the problem. This equation will contain the two variable quantities a and x with their fluxions; then make the coefficient of $\dot{x}$ equal to o, also the coefficient of $\dot{a}$ equal to o. This will give two equations which will determine a and x, and from this we get $b = p - a - x$.

448. Should it be required, in the third place, to find the best course and trim of the sails for getting away from a given line of coast CM (Plate X. fig. 6.), the process perfectly resembles this last, which is in fact getting away from a line of coast which makes a right angle with the wind. Therefore, in place of the angle WCF, we must substitute the angle $WCM \pm WCF$. Call this angle e. We must make $v \cdot$ cos. $(e \pm a \pm b \pm x)$ a maximum. The analyti-

cal process is the same as the former, only c is here a constant quantity.

449. These are the three principal problems which can be solved by means of the knowledge that we have obtained of the motion of the ship when impelled by an oblique sail, and therefore making leeway; and they may be considered as an abstract of this part of M. Bouguer's work. We have only pointed out the process for this solution, and have even omitted some things taken notice of by M. Bezout in his very elegant compendium. Our reasons will appear as we go on. The learned reader will readily see the extreme difficulty of the subject, and the immense calculations which are necessary even in the simplest cases, and will grant that it is out of the power of any but an expert analyst to derive any use from them; but the mathematician can calculate tables for the use of the practical seaman. Thus he can calculate the best position of the sails for advancing in a course 90° from the wind, and the velocity in that course; then for 85°, 80°, 75°, &c. M. Bouguer has given a table of this kind: but to avoid the immense difficulty of the process, he has adapted it to the apparent direction of the wind. We have inserted a few of his numbers, suited to such cases as can be of service, namely, when all the sails draw, or none stand in the way of others. Column 1st is the apparent angle of the wind and course; column 2d is the corresponding angle of the sails and keel; and column 3d is the apparent angle of the sails and wind.

1	2	3
w CF	DCB	w CD
103° 53′	42° 30′	61° 23′
99 13	40 —	50 13
94 25	37 30	56 55
89 28	35 —	54 28
84 23	32 30	51 53
79 06	30 —	49 06
73 39	27 30	46 90
68 —	25 —	43 —

In all these numbers we have the tangent of w CD double of the tangent of DCF.

450. But this is really doing but little for the seaman. The apparent direction of the wind is unknown to him till the ship is sailing with uniform velocity; and he is still uninformed as to the leeway. It is, however, of service to him to know, for instance, that when the angle of the vanes and yards is 56 degrees, the yard should be braced up to 37° 30′, &c.

But here occurs a new difficulty. By the construction of a square-rigged ship it is impossible to give the yards that inclination to the keel which the calculation requires. Few ships can have their yards braced up to 37° 30′; and yet this is required in order to have an incidence of 56°, and to hold a course 94° 25′ from the apparent direction of the wind, that is, with the wind apparently 4° 25′ abaft the beam. A good sailing ship in this position may acquire a velocity even exceeding that of the wind. Let us suppose it only one half of this velocity. We shall find that the angle WC w is in this case about 29°, and the ship is nearly going 123° from the wind, with the wind almost perpendicular to the sail; therefore this utmost bracing up of the sails is only giving them the position suited to a wind broad on the quarter. It is impossible therefore to comply with the demand of the mathematician, and the seaman must be contented to employ a less favourable disposition of his sails in all cases where his course does not lie at least eleven points from the wind.

Let us see whether this restriction, arising from necessity, leaves any thing in our choice, and makes one course preferable to another. We see that there is a prodigious number of courses, and these the most usual and the most important, which we must hold with one trim of the sails; in particular, sailing with the wind on the beam, and all cases of plying to windward, must be performed with this unfavourable trim of the sails. We are certain that the

smaller we make the angle of incidence, real or apparent, the smaller will be the velocity of the ship; but it may happen that we shall gain more to windward, or get sooner away from a lee-coast, or any object of danger, by sailing slowly on one course than by sailing quickly on another.

We have seen that while the trim of the sails remains the same, the leeway and the angle of the yard and course remains the same, and that the velocity of the ship is as the sine of the angle of real incidence, that is, as the sine of the angle of the sail and the real direction of the wind.

Let the ship AB (Plate X. fig. 8.) hold the course CF, with the wind blowing in the direction WC, and having her yards DCD braced up to the smallest angle BCD which the rigging can admit. Let CF be to CE as the velocity of the ship to the velocity of the wind; join FE and draw C *w* parallel to EF; it is evident that FE is the relative motion of the wind, and *w* CD is the relative incidence on the sail. Draw FO parallel to the yard DC, and describe a circle through the points COF; then we say that if the ship, with the same wind and the same trim of the same drawing sails, be made to sail on any other course C *f*, her velocity along CF is to the velocity along C *f* as CF is to C *f*; or, in other words, the ship will employ the same time in going from C to any point of the circumference CFO.

Join *f* O. Then, because the angles CFO, C *f* O are on the same chord CO, they are equal, and *f* O is parallel to *d* C *d*, the new position of the yard corresponding to the new position of the keel *a b*, making the angle *d* C *b* = DCB. Also, by the nature of the circle, the line CF is to C *f* as the sine of the angle COF to the sine of the angle CO *f*, that is (on account of the parallels CD, OF and C *d*, O *f*,) as the sine of WCD to the sine of WC *d*. But when the trim of the sails remains the same, the velocity of the ship is as the sine of the angle of the sail with the direction of the wind; therefore CF is to C *f* as the velocity on CF to that on C *f*, and the proposition is demonstrated.

451. Let it now be required to determine the best course for avoiding a rock R lying in the direction CR, or for withdrawing as fast as possible from a line of coast PQ. Draw CM through R, or parallel to PQ, and let m be the middle of the arch C m M. It is plain that m is the most remote from CM of any point of the arch C m M, and therefore the ship will recede farther from the coast PQ in any given time by holding the course C m than by any other course.

This course is easily determined; for the arch C m M = 360° — (arch CO + arch OM), and the arch CO is the measure of twice the angle CFO, or twice the angle DCB, or twice $\overline{b+x}$, and the arch OM measures twice the angle ECM.

Thus, suppose the sharpest possible trim of the sails to be 35°, and the observed angle ECM to be 70°; then CO+ O M is 70° + 140° or 210°. This being taken from 360°, leaves 150°, of which the half M m is 75°, and the angle MC m is 37° 30′. This added to ECM makes EC m 107° 30′, leaving WC m = 72° 30′, and the ship must hold a course making an angle of 72° 30′ with the real direction of the wind, and WCD will be 37° 30′.

This supposes no leeway. But if we know that under all the sail which the ship can carry with safety and advantage, she makes 5 degrees of leeway, the angle DC m of the sail and course, or $b + x$, is 40°. Then CO + OM = 220°, which being taken from 360° leaves 140°, of which the half is 70°, = M m, and the angle MC m = 35°, and EC m = 105°, and WC m = 75°, and the ship must lie with her head 70° from the wind, making 5 degrees of leeway, and the angle WCD is 35°.

The general rule for the position of the ship is, *that the line on shipboard which bisects the angle* b + x *may also bisect the angle* WCM, or make the angle between the course and the line from which we wish to withdraw equal to the angle between the sail and the real direction of the wind.

452. It is plain that this problem includes that of plying to windward. We have only to suppose ECM to be 90°; then, taking our example in the same ship, with the same trim and the same leeway, we have $b + x = 40°$. This taken from 90° leaves 50° and WC $n = 90 - 25 = 65$, and the ship's head must lie 60° from the wind, and the yard must be 25° from it.

It must be observed here, that it is not always eligible to select the course which will remove the ship fastest from the given line CM; it may be more prudent to remove from it more securely though more slowly. In such cases the procedure is very simple, *viz.* to shape the course as near the wind as is possible.

The reader will also easily see that the propriety of these practices is confined to those courses only where the practicable trim of the sails is not sufficiently sharp. Whenever the course lies so far from the wind that it is possible to make the tangent of the apparent angle of the wind and sail double the tangent of the sail and course, it should be done.

453 These are the chief practical consequences which can be deduced from the theory. But we should consider how far this adjustment of the sails and course can be performed. And here occur difficulties so great as to make it almost impracticable. We have always supposed the position of the surface of the sail to be distinctly observable and measurable; but this can hardly be affirmed even with respect to a sail stretched on a yard. Here we supposed the surface of the sail to have the same inclination to the keel that the yard has. This is by no means the case; the sail assumes a concave form, of which it is almost impossible to assign the direction of the mean impulse. We believe that this is always considerably to leeward of a perpendicular to the yard, lying between CI and CE (Plate X. fig. 6.) This is of some advantage, being equivalent to a sharper trim. We cannot affirm this, however, with any confidence, because it renders

the impulse on the weather-leech of the sail so exceedingly feeble as hardly to have any effect. In sailing close to the wind the ship is kept so near that the weather-leech of the sail is almost ready to receive the wind edgewise, and to flutter or shiver. The most effective or drawing sails with a side-wind, especially when plying to windward, are the staysails. We believe that it is impossible to say, with any thing approaching to precision, what is the position of the general surface of a staysail, or to calculate the intensity and direction of the general impulse; and we affirm with confidence that no man can pronounce on these points with any exactness. If we can guess within a third or a fourth part of the truth, it is all we can pretend to; and after all, it is but a guess. Add to this, the sails coming in the way of each other, and either becalming them or sending the wind upon them in a direction widely different from that of its free motion. All these points we think beyond our power of calculation, and therefore that it is in vain to give the seaman mathematical rules, or even tables of adjustment ready calculated; since he can neither produce that medium position of his sails that is required, nor tell what is the position which he employs.

This is one of the principal reasons why so little advantage has been derived from the very ingenious and promising disquisitions of Bouguer and other mathematicians, and has made us omit the actual solution of the chief problems, contenting ourselves with pointing out the process to such readers as have a relish for these analytical operations.

454. But there is another principal reason for the small progress which has been made in the theory of seamanship: This is the errors of the theory itself, which supposes the impulsions of a fluid to be in the duplicate ratio of the sine of incidence. The most careful comparison which has been made between the results of this theory and matter of fact, is to be seen in the experiments made by the members of the Royal Academy of Sciences at Paris, mentioned in our ar-

ticle on the *Resistance of Fluids*. We subjoin another abstract of them in the following table; where col. 1st gives the angle of incidence; col. 2d gives the impulsions really observed; col. 3d the impulses, had they followed the duplicate ratio of the sines; and col. 4th the impulses, if they were in the simple ratio of the sines.

Angle of Incid.	Impulsion observed.	Impulse as Sine^2.	Impulse as Sine.
90	1000	1000	1000
84	989	989	995
78	958	957	978
72	908	905	951
66	845	835	914
60	771	750	866
54	693	655	809
48	615	552	743
42	543	448	669
36	480	346	587
30	440	250	500
24	424	165	407
18	414	96	309
12	406	43	208
6	400	11	105

Here we see an enormous difference in the great obliquities. When the angle of incidence is only six degrees, the observed impulse is forty times greater than the theoretical impulse; at 12° it is ten times greater; at 18° it is more than four times greater; and at 24° it is almost three times greater.

No wonder then that the deductions from this theory are so useless and so unlike what we familiarly observe. We took notice of this when we were considering the leeway of a rectangular box, and thus saw a reason for admitting an incomparably smaller leeway than what would result from the laborious computations necessary by the theory. This error in theory has as great an influence on the impulsions of air when acting obliquely on a sail; and the experiments of Mr. Robins and of the Chevalier Borda on the oblique

impulsions of air are perfectly conformable (as far as they go) to those of the academicians on water. The oblique impulsions of the wind are therefore much more efficacious for pressing the ship in the direction of her course than the theory allows us to suppose; and the progress of a ship plying to windward is much greater, both because the oblique impulses of the wind are more effective, and because the leeway is much smaller, than we suppose. Were not this the case, it would be impossible for a square-rigged ship to get to windward. The impulse on her sails when close hauled would be so trifling, that she would not have a third part of the velocity which we see her acquire: and this trifling velocity would be wasted in leeway; for we have seen that the diminution of the oblique impulses of the water is accompanied by an increase of leeway. But we see that in the great obliquities the impulsions continue to be very considerable, and that even an incidence of six degrees gives an impulse as great as the theory allows to an incidence of 40. We may therefore, on all occasions, keep the yards more square; and the loss which we sustain by the diminution of the very oblique impulse will be more than compensated by its more favourable direction with respect to the ship's keel. Let us take an example of this. Suppose the wind about two points before the beam, making an angle of 68° with the keel. The theory assigns 43° for the inclination of the wind to the sail, and 25° for the trim of the sail. The perpendicular impulse being supposed 1000, the theoretical impulse for 43° is 465. This reduced in the proportion of radius to the sine of 25°, gives the impulse in the direction of the course only 197.

But if we ease off the lee-braces till the yard makes an angle of 50° with the keel, and allows the wind an incidence of no more than 18°, we have the experimented impulse 414, which, when reduced in the proportion of radius to the sine of 50°, gives an effective impulse 317. In like manner, the trim 56°, with the incidence 12°, gives an effective impulse

337; and the trim 62°, with the incidence only 6°, gives 353.

Hence it would at first sight appear that the angle DCB of 62° and WCD of 6° would be better for holding a course within six points of the wind than any more oblique position of the sails; but it will only give a greater initial impulse. As the ship accelerates, the wind apparently comes ahead, and we must continue to brace up as the ship freshens her way. It is not unusual for her to acquire half or two thirds of the velocity of the wind; in which case the wind comes apparently ahead more than two points, when the yards must be braced up to 35°, and this allows an impulse no greater than about 7°. Now this is very frequently observed in good ships, which in a brisk gale and smooth water will go five or six knots close-hauled, the ship's head six points from the wind, and the sails no more than just full, but ready to shiver by the smallest luff. All this would be impossible by the usual theory; and in this respect these experiments of the French academy give a fine illustration of the seaman's practice. They account for what we should otherwise be much puzzled to explain; and the great progress which is made by a ship close-hauled being perfectly agreeable to what we should expect from the law of oblique impulsion deducible from these so often mentioned experiments, while it is totally incompatible with the common theory, should make us abandon the theory without hesitation, and strenuously set about the establishment of another, founded entirely on experiments. For this purpose the experiments should be made on the oblique impulsions of air on as great a scale as possible, and in as great a variety of circumstances, so as to furnish a series of impulsions for all angles of obliquity. We have but four or five experiments on this subject, *viz.* two by Mr. Robins and two or three by Borda. Having thus gotten a series of impulsions, it is very practicable to raise on this foundation a practical institute, and to give a table of the velocities of a ship suited to every angle

of inclination and of trim; for nothing is more certain than the resolution of the impulse perpendicular to the sail into a force in the direction of the keel, and a lateral force.

456. We are also disposed to think that experiments might be made on a model very nicely rigged with sails, and trimmed in every different degree, which would point out the mean direction of the impulse on the sails, and the comparative force of these impulses in different directions of the wind. The method would be very similar to that for examining the impulse of the water on the hull. If this can also be ascertained experimentally, the intelligent reader will easily see that the whole motion of a ship under sail may be determined for every case. Tables may then be constructed by calculation, or by graphical operations, which will give the velocities of a ship in every different course, and corresponding to every trim of sail. And let it be here observed, that the trim of the sail is not to be estimated in degrees of inclination of the yards; because, as we have already remarked, we cannot observe nor adjust the lateen sails in this way. But, in making the experiments for ascertaining the impulse, the exact position of the tacks and sheets of the sails are to be noted; and this combination of adjustments is to pass by the name of a certain trim. Thus that trim of all the sails may be called 40, whose direction is experimentally found equivalent to a flat surface trimmed to the obliquity 40°.

Having done this, we may construct a figure for each trim similar to Plate X. fig. 8. where, instead of a circle, we shall have a curve COM′F′, whose chords CF′, Cf′, &c. are proportional to the velocities in these courses; and by means of this curve we can find the point m′, which is most remote from any line CM from which we wish to withdraw: and thus we may solve all the principal problems of the art.

457. We hope that it will not be accounted presumption in us to expect more improvement from a theory founded on judicious experiments only, than from a theory of the

impulse of fluids, which is found so inconsistent with observation, and of whose fallacy all its authors, from Newton to D'Alembert, entertained strong suspicions. Again, we beg leave to recommend this view of the subject to the attention of the Society for the Improvement of Naval Architecture. Should these patriotic gentlemen entertain a favourable opinion of the plan, and honour us with their correspondence, we will cheerfully impart to them our notions of the way in which both these trains of experiments may be prosecuted with success, and results obtained in which we may confide; and we content ourselves at present with offering to the public these hints, which are not the speculations of a man of mere science, but of one who, with a competent knowledge of the laws of mechanical nature, has the experience of several years service in the royal navy, where the art of working of ships was a favourite object of his scientific attention.

458. With these observations we conclude our discussion of the first part of the seaman's task, and now proceed to consider the means that are employed to prevent or to produce any deviations from the uniform rectilineal course which has been selected.

Here the ship is to be considered as a body in free space, convertible round her centre of inertia. For whatever may be the point round which she turns, this motion may always be considered as compounded of a rotation round an axis passing through her centre of gravity or inertia. She is impelled by the wind and by the water acting on many surfaces differently inclined to each other, and the impulse on each is perpendicular to the surface. In order therefore that she may continue steadily in one course, it is not only necessary that the impelling forces, estimated in their mean direction, be equal and opposite to the resisting forces estimated in their mean direction; but also that these two directions may pass through one point, otherwise she will be affected as a log of wood is when pushed in opposite direc-

tions by two forces, which are equal indeed, but are applied to different parts of the log. A ship must be considered as a lever acted on in different parts by forces in different directions, and the whole balancing each other round that point or axis where the equivalent of all the resisting forces passes. This may be considered as a point supported by this resisting force, and as a sort of fulcrum: therefore, in order that the ship may maintain her position, the energies or *momenta* of all the impelling forces round this point must balance each other.

459. When a ship sails right afore the wind, with her yards square, it is evident that the impulses on each side of the keel are equal, as also their mechanical *momenta* round any axis passing perpendicularly through the keel. So are the actions of the water on her bows. But when she sails on an oblique course, with her yards braced up on either side, she sustains a pressure in the direction CI (Plate X. fig. 5.) perpendicular to the sail. This, by giving her a lateral pressure LI, as well as a pressure CL ahead, causes her to make leeway, and to move in a line C *b* inclined to CB. By this means the balance of action on the two bows is destroyed; the general impulse on the lee-bow is increased; and that on the weather-bow is diminished. The combined impulse is therefore no longer in the direction BC, but (in the state of uniform motion) in the direction IC.

Suppose that in an instant the whole sails are annihilated, and the impelling pressure CI, which precisely balanced the resisting pressure on the bows, removed. The ship tends by her inertia, to proceed in the direction C *b*. This tendency produces a continuation of the resistance in the opposite direction IC, which is not directly opposed to the tendency of the ship in the direction C *b*; therefore the ship's head would immediately come up to the wind. The experienced seaman will recollect something like this when the sails are suddenly lowered when coming to anchor. It does not happen solely from the obliquity of the action on the

bows: It would happen to the parallelopiped of Plate X. fig. 2. which was sustaining a lateral impulsion $B.\sin.^2 x$, and a direct impulsion $A.\cos.^2 x$. These are continued for a moment after the annihilation of the sail; but being no longer opposed by a force in the direction CD, but by a force in the direction C *b*, the force $B.\sin.^2 x$ must prevail, and the body is not only retarded in its motion, but its head turns towards the wind. But this effect of the leeway is greatly increased by the curved form of the ship's bows. This occasions the centre of effort of all the impulsions of the water on the lee side of the ship to be very far forward, and this so much the more remarkably as she is sharper afore. It is in general not much abaft the foremast. Now the centre of the ship's tendency to continue her motion is the same with her centre of gravity, and this is generally but a little before the mainmast. She is therefore in the same condition nearly as if she were pushed at the mainmast in a direction parallel to C *b*, and at the foremast by a force parallel to IC. The evident consequence of this is a tendency to come up to the wind. This is independent of all situation of the sails, provided only that they have been trimmed obliquely.

460 This tendency of the ship's head to windward is called GRIPING in the seaman's language, and is greatest in ships which are sharp forward, as we have said already. This circumstance is easily understood. Whatever is the direction of the ship's motion, the absolute impulse on that part of the bow immediately contiguous to B is perpendicular to that very part of the surface. The more acute, therefore, that the angle of the bow is, the more will the impulse on that part be perpendicular to the keel, and the greater will be its energy to turn the head to windward.

461. Thus we are enabled to understand or to see the propriety of the disposition of the sails of a ship. We see her crouded with sails forward, and even many sails extended far before her bow, such as the spritsail, the bowsprit topsail, the fore-topmast staysail, the jib, and flying jib. The

sails abaft are comparatively smaller. The sails on the mizenmast are much smaller than those on the foremast. All the staysails hoisted on the mainmast may be considered as headsails, because their centres of effort are considerably before the centre of gravity of the ship; and notwithstanding this disposition, it generally requires a small action of the rudder to counteract the windward tendency of the lee-bow. This is considered as a good quality when moderate; because it enables the seaman to throw the sails aback, and stop the ship's way in a moment, if she be in danger from any thing ahead; and the ship which does not carry a little of a weather helm, is always a dull sailer.

462. In order to judge somewhat more accurately of the action of the water and sails, suppose the ship AB (Plate X. fig. 9.) to have its sails on the mizenmast D, the mainmast E, and foremast F, braced up or trimmed alike, and that the three lines D *i*, E *e*, F *f*, perpendicular to the sails, are in the proportion of the impulses on the sails. The ship is driven ahead and to leeward, and moves in the path *a* C *b*. This path is so inclined to the line of the keel, that the medium direction of the resistance of the water is parallel to the direction of the impulse. A line CI may be drawn parallel to the lines D *i*, E *e*, F *f*, and equal to their sum: and it may be drawn from such a point C, that the actions on all the parts of the hull between C and B may balance the *momenta* of all the actions on the hull between C and A. This point may justly be called the *centre of effort*, or the *centre of resistance*. We cannot determine this point for want of a proper theory of the resistance of fluids. Nay, although experiments like those of the Parisian academy should give us the most perfect knowledge of the intensity of the oblique impulses on a square foot, we should hardly be benefited by them, for the action of the water on a square foot of the hull at *p*, for instance, is so modified by the intervention of the stream of water which has struck the hull about B, and glided along the bow B *o p*, that the pressure

on p is totally different from what it would have been were it a square foot or surface detached from the rest, and presented in the same position to the water moving in the direction b C. For it is found, that the resistances given to planes joined so as to form a wedge, or to curved surfaces, are widely different from the accumulated resistances, calculated for their separate parts, agreeably to the experiments of the academy on single surfaces. We therefore do not attempt to ascertain the point C by theory; but it may be accurately determined by the experiments which we have so strongly recommended; and we offer this as an additional inducement for prosecuting them.

463. Draw through C a line perpendicular to CI, that is, parallel to the sails; and let the lines of impulse of the three sails cut it in the points i, k, and m. This line $i\,m$ may be considered as a lever, moveable round C, and acted on at the points i, k, and m, by three forces. The rotatory momentum of the sails on the mizenmast is $\mathrm{D}\,i \times i\,\mathrm{C}$; that of the sails on the mainmast is $\mathrm{E}\,e \times k\,\mathrm{C}$; and the momentum of the sails on the foremast is $\mathrm{F}\,f \times m\,\mathrm{C}$. The two first tend to press forward the arm C i, and then to turn the ship's head towards the wind. The action of the sails on the foremast tends to pull the arm C m forward, and produce a contrary rotation. If the ship under these three sails keeps steadily in her course, without the aid of the rudder, we must have $\mathrm{D}\,i \times i\,\mathrm{C} + \mathrm{E}\,e \times k\,\mathrm{C} = \mathrm{F}\,f \times m\,\mathrm{C}$. This is very possible, and is often seen in a ship under her mizen-topsail, main-topsail, and fore-topsail, all parallel to one another, and their surfaces duly proportioned by reefing. If more sails are set, we must always have a similar equilibrium. A certain number of them will have their efforts directed from the larboard arm of the lever $i\,m$ lying to leeward of CI, and a certain number will have their efforts directed from the starboard arm lying to windward of CI. The sum of the products of each of the first set, by their distances from C, must be equal to the sum of the similar products of

the other set. As this equilibrium is all that is necessary for preserving the ship's position, and the cessation of it is immediately followed by a conversion; and as these states of the ship may be had by means of the three square sails only, when their surfaces are properly proportioned—it is plain that every movement may be executed and explained by their means. This will greatly simplify our future discussions. We shall therefore suppose in future that there are only the three topsails set, and that their surfaces are so adjusted by reefing, that their actions exactly balance each other round that point C of the middle line AB, where the actions of the water on the different parts of her bottom in like manner balance each other. This point C may be differently situated in the ship according to the leeway she makes, depending on the trim of the sails; and therefore although a certain proportion of the three surfaces may balance each other in one state of leeway, they may happen not to do so in another state. But the equilibrium is evidently attainable in every case, and we therefore shall always suppose it.

464. It must now be observed, that when this equilibrium is destroyed, as, for example, by turning the edge of the mizen-topsail to the wind, which the seamen call *shivering* the mizen-topsail, and which may be considered as equivalent to the removing the mizen-topsail entirely, it does not follow that the ship will turn round the point C, this point remaining fixed. The ship must be considered as a free body, still acted on by a number of forces, which no longer balance each other; and she must therefore *begin* to turn round a spontaneous axis of conversion, which must be determined in the way set forth in the article Rotation. It is of importance to point out in general where this axis is situated. Therefore let G (Plate X. fig. 10.) be the centre of gravity of the ship. Draw the line q G v parallel to the yards, cutting D d in q, E e in r, CI in t, and F f in v. While the three sails are set, the line q v may be considered as a lever acted on by four forces, *viz.* D d, impelling the

lever forward perpendicularly in the point q; E e, impelling it forward in the point r; F f, impelling it forward in the point v; and CI, impelling it backward in the point t. These forces balance each other both in respect of progressive motion and of rotatory energy; for CI was taken equal to the sum of D d, E e, and F f; so that no acceleration or retardation of the ship's progress in her course is supposed.

But by taking away the mizen-topsail, both the equilibriums are destroyed. A part D d of the accelerating force is taken away; and yet the ship, by her inertia or inherent force, tends, for a moment, to proceed in the direction C p with her former velocity; and by this tendency exerts for a moment the same pressure CI on the water, and sustains the same resistance IC. She must therefore be retarded in her motion by the excess of the resistance IC over the remaining impelling forces E e and F f, that is, by a force equal and opposite to D d. She will therefore be retarded in the same manner as if the mizen-topsail were still set, and a force equal and opposite to its action were applied to G the centre of gravity, and she would soon acquire a smaller velocity, which would again bring all things into equilibrium; and she would stand on in the same course, without changing either her leeway or the position of her head.

But the equilibrium of the lever is also destroyed. It is now acted on by three forces only, *viz.* E e and F f, impelling it forward in the points r and v, and IC impelling it backward in the point t. Make $r\,v : r\,o = \mathrm{E}\,e + \mathrm{F}\,f : \mathrm{F}\,f$, and make $o\,p$ parallel to CI and equal to $\mathrm{E}\,e + \mathrm{F}\,f$. Then we know, from the common principles of mechanics, that the force $o\,p$ acting at o will have the same momentum or energy to turn the lever round *any* point whatever as the two forces E e and F f applied at r and v; and now the lever is acted on by two forces, *viz.* IC, urging it backwards in the point t, and $o\,p$ urging it forwards in the point o. It must therefore turn round like a floating log, which gets two

blows in opposite directions. If we now make IC — $o\,p$: $o\,p = t\,o : t\,x$, or IC — $o\,p$: IC = $t\,o : o\,x$, and apply to the point x a force equal to IC — $o\,p$ in the direction IC; we know, by the common principles of mechanics, that this force IC — $o\,p$ will produce the same rotation round any point as the two forces IC and $o\,p$ applied in their proper directions at t and o. Let us examine the situation of the point x.

The force IC — $o\,p$ is evidently = D d, and $o\,p$ is = E e + F f. Therefore $o\,t : t\,x$ = D $d : o\,p$. But because, when all the sails were filled, there was an equilibrium round C, and therefore round t, and because the force $o\,p$ acting at o is equivalent to E e and F f acting at r and v, we must still have the equilibrium; and therefore we have the momentum D $d \times q\,t = o\,p \times o\,t$. Therefore $o\,t : t\,q$ = D $d : o\,p$, and $t\,q = t\,x$. Therefore the point x is the same with the point q.

465. Therefore, when we shiver the mizen-topsail, the rotation of the ship is the same as if the ship were at rest, and a force equal and opposite to the action of the mizen-topsail were applied at q or at D, or at any point in the line D q.

This might have been shown in another and shorter way. Suppose all sails filled, the ship is in equilibrio. This will be disturbed by applying to D a force opposite to D d; and if the force be also equal to D d, it is evident that these two forces destroy each other, and that this application of the force d D is equivalent to the taking away of the mizen-topsail. But we chose to give the whole mechanical investigation; because it gave us an opportunity of pointing out to the reader, in a case of very easy comprehension, the precise manner in which the ship is acted on by the different sails and by the water, and what share each of them has in the motion ultimately produced. We shall not repeat this manner of procedure in other cases, because a little reflection on

the part of the reader will now enable him to trace the *modus operandi* through all its steps.

We now see that, in respect both of progressive motion and of conversion, the ship is affected by shivering the sail D, in the same manner as if a force equal and opposite to D d were applied at D, or at any point in the line D d. We must now have recourse to the principles of rotatory motion.

Let p represent a particle of matter, r its radius vector, or its distance p G from an axis passing through the centre of gravity G, and let M represent the whole quantity of matter of the ship. Then its momentum of inertia is $= \int p.r^2$ (See ROTATION) The ship, impelled in the point D by a force in the direction d D, will begin to turn round a spontaneous vertical axis, passing through a point S of the line q G, which is drawn through the centre of gravity G, perpendicular to the direction d D of the external force, and the distance GS of this axis from the centre of gravity is $= \frac{\int p.r^2}{M \cdot G\, q}$ (see ROTATION,) and it is taken on the opposite side of G from q, that is, S and q are on opposite sides of G.

Let us express the external force by the symbol F. It is equivalent to a certain number of pounds, being the pressure of the wind moving with the velocity V and inclination a on the surface of the sail D; and may therefore be computed either by the theoretical or experimental law of oblique impulses. Having obtained this, we can ascertain the angular velocity of the rotation and the absolute velocity of any given point of the ship by means of the theorems established under ROTATION.

466. But before we proceed to this investigation, we shall consider the action of the rudder, which operates precisely in the same manner. Let the ship AB (Plate X. fig. 11.) have her rudder in the position AD, the helm being hard

a-starboard, while the ship sailing on the starboard tack, and making leeway, keeps on the course *a b*. The lee surface of the rudder meets the water obliquely. The very foot of the rudder meets it in the direction DE parallel to *a b*. The parts farther up meet it with various obliquities, and with various velocities, as it glides round the bottom of the ship and falls into the wake. It is absolutely impossible to calculate the accumulated impulse. We shall not be far mistaken in the deflection of each contiguous filament, as it quits the bottom and glides along the rudder; but we neither know the velocity of these filaments, nor the deflection and velocity of the filaments gliding without them. We therefore imagine that all computations on this subject are in vain. But it is enough for our purpose that we know the direction of the absolute pressure which they exert on its surface. It is in the direction D *d*, perpendicular to that surface. We also may be confident that this pressure is very considerable, in proportion to the action of the water on the ship's bows, or of the wind on the sails; and we may suppose it to be nearly in the proportion of the square of the velocity of the ship in her course; but we cannot affirm it to be accurately in that proportion, for reasons that will readily occur to one who considers the way in which the water falls in behind the ship.

467. It is observed, however, that a fine sailer always steers well, and that all movements by means of the rudder are performed with great rapidity when the velocity of the ship is great. We shall see by and by, that the speed with which the ship performs the angular movements is in the proportion of her progressive velocity: For we shall see that the squares of the times of performing the evolution are as the impulses inversely, which are as the squares of the velocities. There is perhaps no force which acts on a ship that can be more accurately determined by experiment than this. Let the ship ride in a stream or tideway whose velocity is accurately measured; and let her ride from two

moorings, so that her bow may be a fixed point. Let a small tow-line be laid out from her stern or quarter at right angles to the keel, and connected with some apparatus fitted up on shore or on board another ship, by which the strain on it may be accurately measured; a person conversant with mechanics will see many ways in which this can be done. Perhaps the following may be as good as any: Let the end of the tow-line be fixed to some point as high out of the water as the point of the ship from which it is given out, and let this be very high. Let a block with a hook be on the rope, and a considerable weight hung on this hook. Things being thus prepared, put down the helm to a certain angle, so as to cause the ship to sheer off from the point to which the far end of the tow-line is attached. This will stretch the rope, and raise the weight out of the water. Now heave upon the rope, to bring the ship back again to her former position, with her keel in the direction of the stream. When this position is attained, note carefully the form of the rope, that is, the angle which its two parts make with the horizon. Call this angle *a*. Every person acquainted with these subjects knows that the horizontal strain is equal to half the weight multiplied by the cotangent of *a*, or that 2 is to the cotangent of *a* as the weight to the horizontal strain. Now it is this strain which balances and therefore measures the action of the rudder, or D *e* in Plate X. fig. 11. Therefore, to have the absolute impulse D *d*, we must increase D *e* in the proportion of radius to the secant of the angle *b* which the rudder makes with the keel. In a great ship sailing six miles in an hour, the impulse on the rudder inclined 30° to the keel is not less than 3000 pounds. The surface of the rudder of such a ship contains near 80 square feet. It is not, however, very necessary to know this absolute impulse D *d*, because it is its part D *e* alone which measures the energy of the rudder in producing a conversion. Such experiments, made with various positions of the rudder, will give its energies corresponding

to these positions, and will settle that long disputed point which is the best position for turning a ship. On the hypothesis that the impulsions of fluids are in the duplicate ratio of the sines of incidence, there can be no doubt that it should make an angle of 54° 44′ with the keel. But the form of a large ship will not admit of this, because a tiller of a length sufficient for managing the rudder in sailing with great velocity has not room to deviate above 30° from the direction of the keel; and in this position of the rudder the mean obliquity of the filaments of water to its surface cannot exceed 40° or 45°. A greater angle would not be of much service, for it is never for want of a proper obliquity that the rudder fails of producing a conversion.

468. A ship misses stays in rough weather for want of a sufficient progressive velocity, and because her bows are beat off by the waves; and there is seldom any difficulty in wearing the ship, if she has any progressive motion. It is, however, always desirable to give the rudder as much influence as possible. Its surface should be enlarged (especially below) as much as can be done consistently with its strength and with the power of the steersmen to manage it; and it should be put in the most favourable situation for the water to get at it with great velocity; and it should be placed as far from the axis of the ship's motion as possible. These points are obtained by making the stern-post very upright, as has always been done in the French dock-yards. The British ships have a much greater rake; but our builders are gradually adopting the French forms, experience having taught us that their ships, when in our possession, are much more obedient to the helm than our own.—In order to ascertain the motion produced by the action of the rudder, draw from the centre of gravity a line G q perpendicular to D d (D d being drawn through the centre of effort of the rudder.) Then, as in the consideration of the action of the sails, we may conceive the line q G as a lever connected with the ship, and impelled by a force D d acting

perpendicularly at q. The consequence of this will be, an incipient conversion of the ship about a vertical axis passing through some point S in the line q G, lying on the other side of G from q; and we have, as in the former case, GS $= \frac{\int p.r^2}{M \cdot G\, q}$.

469. Thus the action and effects of the sails and of the rudder are perfectly similar, and are to be considered in the same manner. We see that the action of the rudder, though of a small surface in comparison of the sails, must be very great: For the impulse of water is many hundred times greater than that of the wind; and the arm q G of the lever, by which it acts, is incomparably greater than that by which any of the impulsions on the sails produces its effect; accordingly the ship yields much more rapidly to its action than she does to the lateral impulse of a sail.

Observe here, that if G were a fixed or supported axis, it would be the same thing whether the absolute force D d of the rudder acts in the direction D d, or its transverse part D e acts in the direction D e, both would produce the same rotation; but it is not so in a free body. The force D d both tends to retard the ship's motion and to produce a rotation: It retards it as much as if the same force D d had been immediately applied to the centre. And thus the real motion of the ship is compounded of a motion of the centre in a direction parallel to D d, and of a motion round the centre. These two constitute the motion round S.

470. As the effects of the action of the rudder are both more remarkable and somewhat more simple than those of the sails, we shall employ them as an example of the mechanism of the motions of conversion in general; and as we must content ourselves in a work like this with what is very general, we shall simplify the investigation by attending only to the motion of conversion. We can get an accurate notion of the whole motion, if wanted for any purpose, by combin-

ing the progressive or retrograde motion parallel to D d with the motion of rotation which we are about to determine.

In this case, then, we observe, in the first place, that the angular velocity (see ROTATION.) is $\frac{D\,d\cdot q\,G}{\int p r^2}$: and, as was shown in that article, this velocity of rotation increases in the proportion of the time of the forces uniform action, and the rotation would be uniformly accelerated if the forces did really act uniformly. This, however, cannot be the case, because, by the ship's change of position and change of progressive velocity, the direction and intensity of the impelling force is continually changing. But if two ships are performing similar evolutions, it is obvious that the changes of force are similar in similar parts of the evolution. Therefore the consideration of the momentary evolution is sufficient for enabling us to compare the motions of ships actuated by similar forces, which is all we have in view at present.

The velocity v, generated in any time t by the continuance of an invariable momentary acceleration (which is all that we mean by saying that it is produced by the action of a constant accelerating force,) is as the acceleration and the time jointly. Now what we call the *angular velocity* is nothing but this momentary acceleration. Therefore the velocity v generated in the time t is $= \frac{F\cdot q\,G}{\int p r^2} t$.

471. The expression of the angular velocity is also the expresion of the velocity v of a point situated at the distance 1 from the axis G.

Let s be the space or arch of revolution described in the the time t by this point, whose distance from G is $= 1$. Then $\dot{s} = v\,t = \frac{F\cdot q\,G}{\int r^2} t\,\dot{t}$, and taking the fluent $s = \frac{F\cdot q\,G}{\int p r^2} t^2$. This arch measures the whole angle of rotation

accomplished in the time t. These are therefore as the squares of the times from the beginning of the rotation.

Those evolutions are equal which are measured by equal arches. Thus two motions of 45 degrees each are equal. Therefore because z is the same in both, the quantity $\frac{\mathrm{F}\cdot q\,\mathrm{G}}{\int p\,r^2}t^2$ is a constant quantity, and t^2 is reciprocally proportional to $\frac{\mathrm{F}\cdot q\,\mathrm{G}}{\int p\,r^2}$, or is proportional to $\frac{\int p\,r^2}{\mathrm{F}\cdot q\,\mathrm{G}}$, and t is proportional to $\frac{\sqrt{\int p\,r^2}}{\sqrt{\mathrm{F}\cdot q\,\mathrm{G}}}$. That is to say, the times of the similar evolutions of two ships are as the square root of the momentum of inertia directly, and as the square root of the momentum of the rudder or sail inversely. This will enable us to make the comparison easily. Let us suppose the ships perfectly similar in form and rigging, and to differ only in length L and l; $\int \mathrm{P}\cdot\mathrm{R}^2$ is to $\int p\,r^2$ as L^5 to l^5. For the similar particles P and p contain quantities of matter which are as the cubes of their lineal dimensions, that is, as L^3 to l^3. And because the particles are similarly situated, R^2 is to r^2 as L^2 to l^2. Therefore $\mathrm{P}\cdot\mathrm{R}^2 : p\cdot r^2 = \mathrm{L}^5 : l^5$. Now F is to f as L^2 to l^2. For the surfaces of the similar rudders or sails are as the squares of their lineal dimensions, that is, as L^2 to l^2. And, lastly, G q is to $g\,q$ as L to l, and therefore $\mathrm{F}\cdot\mathrm{G}\,q : f\cdot g\,q = \mathrm{L}^3 : l^3$. Therefore we have $\mathrm{T}^2 : t^2 = \frac{\int \mathrm{P}\cdot\mathrm{R}^2}{\mathrm{F}\cdot\mathrm{G}\,q} : \frac{\int p\cdot r_2}{f\cdot g\,q} = \frac{\mathrm{L}^5}{\mathrm{L}^3} : \frac{l^5}{l^3} = \mathrm{L}^2 : l^2$, and $\mathrm{T} : t = \mathrm{L} : l$.

472. Therefore the times of performing similar evolutions with similar ships are proportional to the lengths of the ships when both are sailing equally fast; and since the evolutions are similar, and the forces vary similarly in their different

parts, what is here demonstrated of the smallest incipient evolutions is true of the whole. They therefore not only describe equal angles of revolution, but also similar curves.

A small ship, therefore, works in less time and in less room than a great ship, and this in the proportion of its length. This is a great advantage in all cases, particularly in wearing, in order to sail on the other tack close-hauled. In this case she will always be to windward and ahead of the large ship, when both are got on the other tack. It would appear at first sight that the large ship will have the advantage in tacking. Indeed the large ship is farther to windward when again trimmed on the other tack than the small ship when she is just trimmed on the other tack. But this happened before the large ship had completed her evolution, and the small ship, in the mean time, has been going forward on the other tack, and going to windward. She will therefore be before the large ship's beam, and perhaps as far to windward.

473. We have seen that the velocity of rotation is proportional, *cæteris paribus*, to $F \times G\ q$. F means the absolute impulse on the rudder or sail, and is always perpendicular to its surface. This absolute impulse on a sail depends on the obliquity of the wind to its surface. The usual theory says, that it is as the square of the sine of incidence: but we find this not true. We must content ourselves with expressing it by some as yet unknown function φ of the angle of incidence a, and call it $\varphi\ a$; and if S be the surface of the sail, and V the velocity of the wind, the absolute impulse is $n\ V^2\ S \times \varphi\ a$. This acts (in the case of the mizen-topsail, Plate X. fig. 10.) by the lever q G, which is equal to DG $\times$ cos. DG q, and DG q is equal to the angle of the yard and keel; which angle we formerly called b. Therefore its energy in producing a rotation is $n\ V^2\ S \times \varphi\ a \times DG \times \cos.\ b$. Leaving out the constant quantities n, V^2, S, and DG, its energy is proportional to $\varphi\ a \times \cos.\ b$. In order, therefore, that any sail may have the greatest power to produce a ro-

tation round G, it must be so trimmed that $\varphi\ a \times \cos.\ b$ may be a maximum. Thus, if we would trim the sails on the foremast, so as to pay the ship off from the wind right ahead with the greatest effect, and if we take the experiments of the French academicians as proper measures of the oblique impulses of the wind on the sail, we will brace up the yard to an angle of 48 degrees with the keel. The impulse corresponding to 48° is 615, and the cosine of 48° is 669. These give a product of 411435. If we brace the sail to 54° 44′, the angle assigned by the theory, the effective impulse is 405274. If we make the angle 45°, the impulse is 408774. It appears then that 48° is preferable to either of the others. But the difference is inconsiderable, as in all cases of maximum a small deviation from the best position is not very detrimental. But the difference between the theory and this experimental measure will be very great when the impulses of the wind are of necessity very oblique. Thus, in tacking ship, as soon as the headsails are taken aback, they serve to aid the evolution, as is evident: But if we were now to adopt the maxim inculcated by the theory, we should immediately round in the weather-braces so as to increase the impulse on the sail, because it is then very small; and although we by this means make yard more square, and therefore diminish the rotatory momentum of this impulse, yet the impulse is more increased (by the theory) than its vertical lever is diminished.—Let us examine this a little more particularly, because it is reckoned one of the nicest points of seamanship to aid the ship's coming round by means of the headsails; and experienced seamen differ in their practice in this manœuvre. Suppose the yard braced up to 40°, which is as much as can be usually done, and that the sail shivers (the bowlines are usually let go when the helm is put down), the sail immediately takes aback, and in a moment we may suppose an incidence of 6 degrees. The impulse corresponding to this is 400 (by experiment), and the cosine of 40° is 766. This gives 306400 for the effective impulse. To

proceed according to the theory, we should brace the yard to 70°, which would give the wind (now 34° on the weather-bow) an incidence of nearly 36°, and the sail an inclination of 20° to the intended motion, which is perpendicular to the keel. For the tangent of 20° is about $\frac{1}{2}$ of the tangent of 36°. Let us now see what effective impulse the experimental law of oblique impulsions will give for this adjustment of the sails. The experimental impulse for 36° is 480; the cosine of 70° is 342; the product is 164160, not much exceeding the half of the former. Nay, the impulse for 36°, calculated by the theory, would have been only 346, and the effective impulse only 118332. And it must be farther observed, that this theoretical adjustment would tend greatly to check the evolution, and in most cases would entirely mar it, by checking the ship's motion ahead, and consequently the action of the rudder, which is the most powerful agent in the evolution; for here would be a great impulse directed almost astern.

We were justifiable, therefore, in saying, in the beginning of this article, that a seaman would frequently find himself baffled if he were to work a ship according to the rules deduced from M. Bouguer's work; and we see by this instance of what importance it is to have the oblique impulsions of fluids ascertained experimentally. The practice of the most experienced seamen is directly the opposite to this theoretical maxim, and its success greatly confirms the usefulness of these experiments of the academicians so often praised by us.

We return again to the general consideration of the rotatory motion. We found the velocity $v = \frac{F \cdot q\,G}{\int p\,r^2}$. It is therefore proportional, *cæteris paribus*, to q G. We have seen in what manner q G depends on the position and situation of the sail or rudder when the point G is fixed. But it also depends on the position of G. With respect to the ac-

tion of the rudder, it is evident that it is so much the more powerful as it is more remote from G. The distance from G may be increased either by moving the rudder farther aft, or G farther forward. And as it is of the utmost importance that a ship answer her helm with the greatest promptitude, those circumstances have been attended to which distinguished fine steering ships from such as had not this quality; and it is in a great measure to be ascribed to this, that, in the gradual improvement of naval architecture, the centre of gravity has been placed far forward. Perhaps the notion of a centre of gravity did not come into the thoughts of the rude builders in early times; but they observed that those boats and ships steered best which had their extreme breadth before the middle point, and consequently the bows not so acute as the stern. This is so contrary to what one would expect, that it attracted attention more forcibly; and, being somewhat mysterious, it might prompt to attempts of improvement, by exceeding in this singular maxim. We believe that it has been carried as far as is compatible with other essential requisites in a ship.

474. We believe that this is the chief circumstance in what is called the trim of a ship; and it were greatly to be wished that the best place for the centre of gravity could be accurately ascertained. A practice prevails, which is the opposite of what we are now advancing. It is usual to load a ship so that her keel is not horizontal, but lower abaft. This is found to improve her steerage. The reason of this is obvious. It increases the acting surface of the rudder, and allows the water to come at it with much greater freedom and regularity; and it generally diminishes the griping of the ship forward, by removing a part of the bows out of the water. It has not always this effect; for the form of the harping aloft is frequently such, that the tendency to gripe is diminished by immersing more of the bow in the water.

But waving these circumstances, and attending only to the rotatory energy of the rudder, we see that it is of advantage to carry the centre of gravity forward. The same advantage is gained to the action of the after sails. But, on the other hand, the action of the headsails is diminished by it; and we may call every sail a headsail whose centre of gravity is before the centre of gravity of the ship; that is, all the sails hoisted on the bowsprit and foremast, and the staysails hoisted on the mainmast; for the centre of gravity is seldom far before the mainmast.

Suppose that when the rudder is put into the position AD (Plate X. fig. 11.), the centre of gravity could be shifted to g, so as to increase q G, and that this is done without increasing the sum of the products $p\,r^2$. It is obvious that the velocity of conversion will be increased in the proportion of q G to $q\,g$. This is very possible, by bringing to that side of the ship parts of her loading which were situated at a distance from G on the other side. Nay, we can make this change in such a manner that $\int p\,r^2$ shall even be less than it was before, by taking care that every thing which we shift shall be nearer to g than it was formerly to G. Suppose it all placed in one spot m, and that m is the quantity of matter so shifted, while M is the quantity of matter in the whole ship. It is only necessary that $m{\cdot}g\ \mathrm{G}^2$ shall be less than the sum of the products $p\,r^2$ corresponding to the matter which has been shifted. Now, although the matter which is easily moveable is generally very small in comparison to the whole matter of the ship, and therefore can make but a small change in the place of the centre of gravity, it may frequently be brought from places so remote, that it may occasion a very sensible diminution of the quantity $\int p\,r^2$, which expresses the whole momentum of inertia.

475. This explains a practice of the seamen in small wherries or skiffs, who in putting about are accustomed to place

themselves to leeward of the mast. They even find that they can aid the quick motions of these light boats by the way in which they rest on their two feet, sometimes leaning all on one foot, and sometimes on the other. And we have often seen this evolution very sensibly accelerated in a ship of war, by the crew running suddenly, as the helm is put down, to the lee-bow. And we have heard it asserted by very expert seamen, that after all attempts to wear ship (after lying-to in a storm) have failed, they have succeeded by the crew collecting themselves near the weather fore-shrouds the moment the helm was put down. It must be agreeable to the reflecting seaman to see this practice supported by undoubted mechanical principles.

476. It will appear paradoxical to say that the evolution may be accelerated even by an addition of matter to the ship; and though it is only a piece of curiosity, our readers may wish to be made sensible of it. Let m be the addition, placed in some point m lying beyond G from q. Let S be the spontaneous centre of conversion before the addition. Let v be the velocity of rotation round g, that is, the velocity of a point whose distance from g is 1, and let ϱ be the radius vector, or distance of a particle from g. We have (Rotation) $v = \dfrac{F \cdot q\,g}{\int p\,\varrho^2 + m \cdot m\,g^2}$. But we know (Rotation) that $\int p\,\varrho^2 = \int p\,r^2 + M \cdot G\,g^2$. Therefore $v = \dfrac{F \cdot q\,g}{\int p\,r^2 + M \cdot G\,g^2 + m \cdot m\,g^2}$. Let us determine G g and $m\,g$ and $q\,g$.

Let m G be called z. Then, by the nature of the centre of gravity, $M + m : M = G\,m : g\,m = z : g\,m$, and $g\,m = \dfrac{M}{M+m}\,z$, and $m \cdot g\,m^2 = \dfrac{m\,M^2}{M+m^2}\,z^2$. In like manner, $M \cdot G\,g^2 = \dfrac{M\,m^2}{M+m^2}\,z^2$. Now $m\,M^2 + M\,m^2 =$

$M\,m \times M + m$. Therefore $M \cdot G\,g^2 + m \cdot g\,m^2 = \frac{M\,m \times (M+m)}{M+m,} z^2, = \frac{M\,m}{M+m} z^2$. Let n be $= \frac{m}{M+m}$, then $M.\,G\,g^2 + m \cdot g\,m^2 = M\,n\,z^2$. Also $G\,g = n\,z$, being $= \frac{m}{M+n} z$. Let q G be called c: then $q\,g = c + n\,z$. Also let SG be called e.

We have now for the expression of the velocity $v = \frac{F\,(c+n\,z)}{\int p\,r^2 + M\,n\,z^2}$, or $v = \frac{F}{M} \times \frac{c+n\,z}{\int \frac{p\,r^2+nz^2}{M}}$. But (ROTATION) $\int \frac{p\,r^2}{M} = c\,e$. Therefore, finally, $v = \frac{F}{M} \times \frac{c+n\,z}{c\,e+n\,z^2}$. Had there been no addition of matter made, we should have had $v = \frac{F}{M} \times \frac{c}{c\,e}$. It remains to show, that z may be so taken that $\frac{c}{c\,e}$ may be less than $\frac{c+n\,z}{ce+nz^2}$. Now, if c be to z as $c\,e$ to z^2, that is, if z be taken equal to e, the two fractions will be equal. But if z be less than e, that is, if the additional matter is placed anywhere between S and G, the complex fraction will be greater than the fraction $\frac{c}{c\,e}$, and the velocity of rotation will be increased. There is a particular distance which will make it the greatest possible, namely, when z is made $= \frac{1}{n}(\sqrt{c^2 + n\,c\,e} - c)$, as will easily be found by treating the fraction $\frac{c+n\,z}{c\,e+n\,z^2}$, with z, considered as the variable quantity, for a maximum. In what we have been saying on this subject, we have considered the rotation only in as much as it is performed round the centre of gravity, although in every moment it is really performed round a spontaneous axis lying beyond that centre. This was done

because it afforded an easy investigation, and any angular motion round the centre of gravity is equal to the angular motion round any other point. Therefore the extent and the time of the evolution are accurately defined.—From observing that the energy of the force F is proportional to q G, an inattentive reader will be apt to conceive the centre of gravity as the centre of motion, and the rotation as taking place because the momenta of the sails and rudder, on the opposite sides of the centre of gravity, do not balance each other. But we must always keep in mind that this is not the cause of the rotation. The cause is the want of equilibrium round the point C (Plate X. fig. 10.), where the actions of the water balance each other. During the evolution, which consists of a rotation combined with a progressive motion, this point C is continually shifting, and the unbalanced momenta which continue the rotation always respect the momentary situation of the point C. It is nevertheless always true that the energy of a force F is proportional *(cæteris paribus)* to q G, and the rotation is always made in the same direction as if the point G were really the centre of conversion. Therefore the mainsail acts always (when oblique) by pushing the stern away from the wind, although it should sometimes act on a point of the vertical lever through C, which is a-head of C.

These observations on the effects of the sails and rudder in producing a conversion, are sufficient for enabling us to explain any case of their action which may occur. We have not considered the effects which they tend to produce by inclining the ship round a horizontal axis, viz. the motions of rolling and pitching. To treat this subject properly would lead us into the whole doctrine of the equilibrium of floating bodies, and it would rather lead to maxims of construction than to maxims of manœuvre. M. Bouguer's *Traité du Navire* and Euler's *Scientia Navalis* are excellent performances on this subject, and we are not here obliged to have recourse to any erroneous theory.

477. It is easy to see that the lateral pressure both of the wind on the sails and of the water on the rudder tends to incline the ship to one side. The sails also tend to press the ship's bows into the water, and, if she were kept from advancing, would press them down considerably. But by the ship's motion, and the prominent form of her bows, the resistance of the water to the fore part of the ship produces a force which is directed upwards. The sails also have a small tendency to raise the ship, for they constitute a surface which in general separates from the plumb-line below. This is remarkably the case in the staystails, particularly the jib and fore-topmast staysail. And this helps greatly to soften the plunges of the ship's bows into the head seas. The upward pressure also of the water on her bows, which we just now mentioned, has a great effect in opposing the immersion of the bows which the sails produce by acting on the long levers furnished by the masts. M. Bouguer gives the name of *point velique* to the point V. (Plate X. fig. 12.) of the mast, where it is cut by the line CV, which marks the mean place and direction of the whole impulse of the water on the bows. And he observes, that if the mean direction of all the actions of the wind on the sails be made to pass also through this point, there will be a perfect equilibrium, and the ship will have no tendency to plunge into the water or to rise out of it; for the whole action of the water on the bows, in the direction CV, is equivalent to, and may be resolved into the action CE, by which the progressive motion is resisted, and the vertical action CD, by which the ship is raised above the water. The force CE must be opposed by an equal force VD, exerted by the wind on the sails, and the force CD is opposed by the weight of the ship. If the mean effort of the sails passes above the point V, the ship's bows will be pressed into the water; and if it pass below V, her stern will be pressed down. But, by the union of these forces, she will rise and fall with the sea, keeping always in a parallel position. We apprehend that it is of very little

moment to attend to the situation of this point. Except when the ship is right afore the wind, it is a thousand chances to one that the line CV of mean resistance does not pass through any mast; and the fact is, that the ship cannot be in a state of uniform motion on any other condition but the perfect union of the line of mean action of the sails, and the line of mean action of the resistance. But its place shifts by every change of leeway or of trim; and it is impossible to keep these lines in one constant point of intersection for a moment, on account of the incessant changes of the surface of the water on which she floats. M. Bouguer's observations on this point are, however, very ingenious and original.

We conclude this dissertation, by describing some of the chief movements or evolutions. What we have said hitherto is intended for the instruction of the artist, by making him sensible of the mechanical procedure. The description is rather meant for the amusement of the landsman, enabling him to understand operations that are familiar to the seaman. The latter will perhaps smile at the awkward account given of his business by one who cannot hand, reef, or steer.

To tack Ship.

The ship must first be kept full, that is, with a very sensible angle of incidence on the sails, and by no means hugging the wind. For, as this evolution is chiefly performed by the rudder, it is necessary to give the ship a good velocity. When the ship is observed to luff up of herself, that moment is to be catched for beginning the evolution, because she will by her inherent force continue this motion. The helm is then put down. When the officer calls out Helm's a-lee, the fore-sheet, fore-top bowline, jib, and flag-sail sheets forward are let go. The jib is frequently hauled down. Thus the obstacles to the ship's head coming

up to the wind by the action of the rudder are removed. If the mainsail is set, it is not unusual to clue up the weather side, which may be considered as a headsail, because it is before the centre of gravity. The mizen must be hauled out, and even the sail braced to windward. Its power in paying off the stern from the wind conspires with the action of the rudder. It is really an aerial rudder. The sails are immediately taken aback. In this state the effect of the mizen-topsail would be to obstruct the movement, by pressing the stern the contrary way to what it did before. It is therefore either immediately braced about sharp on the other tack, or lowered. Bracing it about evidently tends to pay round the stern from the wind, and thus assist in bringing the head up to the wind. But in this position it checks the progressive motion of the ship, on which the evolution chiefly depends. For a rapid evolution, therefore, it is as well to lower the mizen-topsail. Meantime, the headsails are all aback, and the action of the wind on them tends greatly to pay the ship round. To increase this effect, it is not unusual to haul the foretop bowline again. The sails on the mainmast are now almost becalmed; and therefore, when the wind is right ahead, or a little before, the mainsail is hauled round and braced up sharp on the other tack with all expedition. The staysail sheets are now shifted over to their places for the other tack. The ship is now entirely under the power of the headsails and of the rudder, and their actions conspire to promote the conversion. The ship has acquired an angular motion, and will preserve it, so that now the evolution is secured, and she falls off apace from the wind on the other tack. The farther action of the rudder is therefore unnecessary, and would even be prejudicial, by causing the ship to fall off too much from the wind before the sails can be shifted and trimmed for sailing on the other tack. It is therefore proper to right the helm when the wind is right a-head, that is, to bring the rudder into the direction of the

keel. The ship continues her conversion by her inherent force and the action of the headsails.

When the ship has fallen off about four points from the wind, the headsails are hauled round and trimmed sharp on the other tack with all expedition; and although this operation was begun with the wind four points on the bow, it will be six before the sails are braced up, and therefore the headsails will immediately fill. The after-sails have filled already, while the headsails were inactive, and therefore immediately check the farther falling off from the wind. All sails now draw, for the staysail sheets have been shifted over while they were becalmed or shaking in the wind. The ship now gathers way, and will obey the smallest motion of the helm to bring her close to the wind.

We have here supposed, that during all this operation the ship preserves her progressive motion. She must there fore have described a curve line, advancing all the while to windward. Fig. 13. is a representation of this evolution when it is performed in the completest manner. The ship standing on the course E *a*, with the wind blowing in the direction WF, has her helm put hard a-lee when she is in the position A. She immediately deviates from her course, and describing a curve, comes to the position B, with the wind blowing in the direction WF of the yards, and the square-sails now shiver. The mizen topsail is here represented braced sharp on the other tack, by which its tendency to aid the angular motion (while it checks the progressive motion) is distinctly seen. The main and foresails are now shivering, and immediately after are taken aback. The effect of this on the headsails is distinctly seen to be favourable to the conversion, by pushing the point F in the direction F *i*; but for the same reason it continues to retard the progressive motion. When the ship has attained to the position C, the mainsail is hauled round and trimmed for the other tack. The impulse in the direction F *i* still aids the conversion and retards the progressive motion.

When the ship has attained a position between C and D, such that the main and mizen-topsail yards are in the direction of the wind, there is nothing to counteract the force of the headsails to pay the ship's head off from the wind. Nay, during the progress of the ship to this intermediate position, if any wind gets at the main or mizen topsails, it acts on their anterior surfaces, and impels the after parts of the ship away from the curve *a b c d*, and thus aids the revolution. We have therefore said, that when once the sails are taken fully aback, and particularly when the wind is brought right ahead, it is scarce possible for the evolution to fail; as soon therefore as the main-topsail (trimmed for the other tack) shivers, we are certain that the headsails will be filled by the time they are hauled round and trimmed. The staysails are filled before this, because their sheets have been shifted, and they stand much sharper than the square-sails; and thus every thing tends to check the falling off from the wind on the other tack, and this no sooner than it should be done. The ship immediately gathers way, and holds on in her new course *d* G.

But it frequently happens, that in this conversion the ship loses her whole progressive motion. This sometimes happens while the sails are shivering before they are taken fully aback. It is evident, that in this case there is little hopes of success, for the ship now lies like a log, and neither sails nor rudder have any action. The ship drives to leeward like a log, and the water acting on the lee side of the rudder checks a little the driving of the stern. The head therefore falls off again, and by and by the sails fill, and the ship continues on her former tack. This is called MISSING STAYS, and it is generally owing to the ship's having too little velocity at the beginning of the evolution. Hence the propriety of keeping the sails well filled for some little time before. Rough weather, too, by raising a wave which beats violently on the weather-bow, frequently checks the first luffing of the ship, and beats her off again.

If the ship lose all her motion after the headsails have been fully taken aback, and before we have brought the wind right ahead, the evolution becomes uncertain, but by no means desperate; for the action of the wind on the headsails will presently give her stern-way. Suppose this to happen when the ship is in the position C. Bring the helm over hard to windward, so that the rudder shall have the position represented by the small dotted line *o f*. It is evident, that the resistance of the water to the stern-way of the rudder acts in a favourable direction, pushing the stern outwards. In the meantime, the action of the wind on the headsails pushes the head in the opposite direction. These actions conspire therefore in promoting the evolution; and if the wind is right ahead, it cannot fail, but may even be completed speedily, because the ship gathers stern-way, and the action of the rudder becomes very powerful; and as soon as the wind comes on the formerly lee-bow, the action of the water on the now lee-quarter will greatly accelerate the conversion. When the wind therefore has once been brought nearly right ahead, there is no risk of being baffled.

But should the ship have lost all her head-way considerably before this, the evolution is very uncertain; for the action of the water on the rudder may not be nearly equal to its contrary action on the lee-quarter; in which case the action of the wind on the headsails may not be sufficient to make up the difference. When this is observed, when the ship goes astern without changing her position, we must immediately throw the headsails completely aback, and put the helm down again, which will pay off the ship's head from the wind enough to enable us to fill the sails again on the same tack, to try our fortune again; or we must BOXHAUL the ship, in the manner to be described by and by.

Such is the ordinary process of tacking ship,—a process in which all the different modes of action of the rudder and

sails are employed. To execute this evolution in the most expeditious manner, and so as to gain as much on the wind as possible, is considered as the test of an expert seaman. We have described the process which is best calculated for answering the purpose. But if the ship be sailing very briskly in smooth water, so that there is no danger of missing stays, we may gain more to windward considerably by keeping fast the fore-top bowline and jib and stay-sail sheets till the square-sails are all shivering: for these sails, continuing to draw with considerable force, and balancing each other nobably fore and aft, keep up the ship's velocity very much, and thus maintain the power of the rudder. If we now let all fly when the square sails are shivering, the ship may be considered as without sails, but exposed to the action of the water on the lee-bow; from which arises a strong pressure of the bow to windward which conspires with the action of the rudder to aid the conversion. It evidently leaves all that tendency of the bow to windward which arises from leeway, and even what was counteracted by the formerly unbalanced action of these head-sails. This method lengthens the whole time of the evolution, but it advances the ship to windward. Observe, too, that keeping fast the foretop bowline till the sail shivers, and then letting it go, ensures the taking aback of that sail, and thus instantly produces an action that is favourable to the evolution.

The most expert seamen, however, differ among themselves with respect to these two methods, and the first is the most generally practised in the British navy, because the least liable to fail. The forces which oppose the conversion are sooner removed, and the production of a favourable action by the backing of the foretop-sail is also sooner obtained, by letting go the foretop bowline at the first.

Having entered so minutely into the description and rationale of this evolution, we have sufficiently turned the

reader's attention to the different actions which co-operate in producing the motions of conversion. We shall therefore be very brief in our description of the other evolutions.

To wear Ship.

When the seaman sees that his ship will not go about head to wind, but will miss stays, he must change his tack the other way; that is, by turning her head away from the wind, going a little way before the wind, and then hauling the wind on the other tack. This is called wearing or veering ship. It is most necessary in stormy weather with little sail, or in very faint breezes, or in a disabled ship.

The process is exceedingly simple; and the mere narration of the procedure is sufficient for showing the propriety of every part of it.

Watch for the moment of the ship's falling off, and then haul up the mainsail and mizen, and shiver the mizen-topsail, and put the helm a-weather. When the ship falls off sensibly (and not before), let go the bowlines. Ease away the fore-sheet, raise the fore-tack, and gather aft the weather fore-sheet as the lee-sheet is eased away. Round in the weather-braces of the fore and main-masts, and keep the yards nearly bisecting the angle of the wind and keel, so that when the ship is before the wind the yards may be square. It may even be of advantage to round in the weather-braces of the main-topsail more than those of the headsails; for the mainmast is abaft the centre of gravity. All this while the mizen-topsail must be kept shivering, by rounding in the weather-braces as the ship pays off from the wind. Then the main-topsail will be braced up for the other tack by the time that we have brought the wind on the weather quarter. After this it will be full, and will aid the evolution. When the wind is right aft, shift the jib and staysail sheets. The evolution now goes on with great

rapidity: therefore briskly haul on board the fore and main tacks, and haul out the mizen, and set the mizen-staysails as soon as they will take the wind the right way. We must now check the great rapidity with which the ship comes to the wind on the other tack, by righting the helm before we bring the wind on the beam; and all must be trimmed sharp fore and aft by this time, that the head-sails may take and check the coming-to. All being trimmed, stand on close by the wind.

We cannot help losing much ground in this movement. Therefore, though it be very simple, it requires much attention and rapid execution to do it with as little ground as possible. One is apt to imagine at first that it would be better to keep the headsails braced up on the former tack, or at least not to round in the weather-braces so much as is here directed. When the ship is right afore the wind, we should expect assistance from the obliquity of the head-sails; but the rudder being the principal agent in the evolution, it is found that more is gained by increasing the ship's velocity, than by a smaller impulse in the headsails more favourably directed. Experienced seamen differ, however, in their practice in respect of this particular.

To box haul a Ship.

This is a process performed only in critical situations, as when a rock, a ship, or some danger, is suddenly seen right a-head, or when a ship misses stays. It requires the most rapid execution.

The ship being close hauled on a wind, haul up the main-sail and mizen, and shiver the topsails, and put the helm hard a-lee altogether. Raise the fore-tack, let go the head bowlines, and brace about the headsails sharp on the other tack. The ship will quickly lose her way, get stern-way, and then fall off, by the joint action of the headsails and of the inverted rudder. When she has fallen off eight points,

brace the aftersails square, which have hitherto been kept shivering. This will at first increase the power of the rudder, by increasing the stern-way, and at the same time it makes no opposition to the conversion which is going on. The continuation of her circular motion will presently cause them to take the wind on their after surfaces. This will check the stern-way, stop it, and give the ship a little head-way. Now shift the helm, so that the rudder may again act in conjunction with the headsails in paying her off from the wind. This is the critical part of the evolution, because the ship has little or no way through the water, and will frequently remain long in this position. But as there are no counteracting forces, the ship continues to fall off. Then the weather-braces of the after-sails may be gently rounded in, so that the wind acting on their hinder surfaces may both push the ship a little a-head and her stern laterally in conjunction with the rudder. Thus the wind is brought upon the quarter and the headsails shiver. By this time the ship has acquired some head-way. A continuation of the rotation would now fill the headsails, and their action would be contrary to the intended evolution. They are therefore immediately braced the other way, nearly square, and the evolution is now completed in the same manner with wearing ship.

Some seamen brace all the sails aback the moment that the helm is put hard a-lee, but the after-sails no more aback than just to square the yards. This quickly gives the ship stern-way, and brings the rudder into action in its inverted direction; and they think that the evolution is accelerated by this method.

There is another problem of seamanship deserving of our attention, which cannot properly be called an evolution. This is lying to. This is done in general by laying some sails aback, so as to stop the head-way produced by others. But there is a considerable address necessary for doing this in such a way that the ship shall lie easily, and under com-

mand, ready to proceed in her course, and easily brought under weigh.

To bring to with the fore or main topsail to the mast, brace that sail sharp aback, haul out the mizen, and clap the helm hard a-lee.

Suppose the fore-topsail to be aback, the other sails shoot the ship a-head, and the lee-helm makes the ship come up to the wind, which makes it come more perpendicularly on the sail which is aback. Then its impulse soon exceeds those on the other sails, which are now shivering or almost shivering. The ship stands still awhile, and then falls off, so as to fill the after-sails, which again shoot her a-head, and the process is thus repeated. A ship lying to in this way goes a good deal a-head and also to leeward. If the main-topsail be aback, the ship shoots a-head, and comes up till the diminished impulse of the drawing sails in the direction of the keel is balanced by the increased impulse on the main topsail. She lies a long while in this position driving slowly to leeward; and she at last falls off by the beating of the water on her weather-bow. She falls off but little, and soon comes up again.

Thus a ship lying to is not like a mere log, but has a certain motion which keeps her under command. To get under weigh again, we must watch the time of falling off; and when this is just about to finish, brace about briskly, and fill the sail which was aback. To aid this operation, the jib and foretopmast staysail may be hoisted, and the mizen brailed up: or, when the intended course is before the wind or large, back the foretopsail sharp, shiver the main and mizen topsail, brail up the mizen, and hoist the jib and foretopmast stay sails altogether.

In a storm with a contrary wind, or on a lee shore, a ship is obliged to lie to under a very low sail. Some sail is absolutely necessary, in order to keep the ship steadily down, otherwise she would kick about like a cork, and roll so deep as to strain and work herself to pieces. Different ships

behave best under different sails. In a very violent gale, the three lower staysails are in general well adapted for keeping her steady, and distributing the strain. This mode seems also well adapted for wearing, which may be done by hauling down the mizen-staysail. Under whatever sail the ship is brought to in a storm, it is always with a fitted sail, and never with one laid aback. The helm is lashed down hard a-lee; therefore the ship shoots a-head, and comes up till the sea on her weather-bow beats her off again. Getting under weigh is generally difficult; because the ship and rigging are lofty abaft, and hinder her from falling off readily when the helm is put hard a-weather. We must watch the falling off, and assist the ship by some small headsail. Sometimes the crew get up on the weather fore-shrowds in a crowd, and thus present a surface to the wind.

These examples of the three chief evolutions will enable those who are not seamen to understand the propriety of the different steps, and also to understand the other evolutions as they are described by practical authors. We are not acquainted with any performance in our language where the whole are considered in a connected and systematic manner. There is a book on this subject in French, called *La Manœuvrier*, by M. Burdé de Ville-Huet, which is in great reputation in France. A translation into English was published some years ago, said to be the performance of the Chevalier de Sauseuil, a French officer. But this is undoubtedly the work of some person who did not understand either the French language, or the subject, or the mathematical principles which are employed in the scientific part. The blunders are not such as could possibly be made by a Frenchman not versant in the English language, but natural for an Englishman ignorant of French. No French gentleman or officer would have translated a work of this kind (which he professes to think so

highly of) to serve the rivals and foes of his country. But indeed it can do no great harm in this way; for the scientific part of it is absolutely unintelligible for want of science in the translator; and the practical part is full of blunders for want of knowledge of the French language.

We offer this account of the subject with all proper respect and diffidence. We do not profess to teach; but, by pointing out the defects of the celebrated works of M. Bouguer, and the course which may be taken to remove them, while we preserve much valuable knowledge which they contain, we may perhaps excite some persons to apply to this subject, who, by a combination of what is just in M. Bouguer's theory, with an experimental doctrine of the impulses of fluids, may produce a treatise of seamanship which will not be confined to the libraries of mathematicians, but become a manual for seamen by profession.

END OF VOLUME FOURTH.

Q160. R66 V4

a39001 004859666